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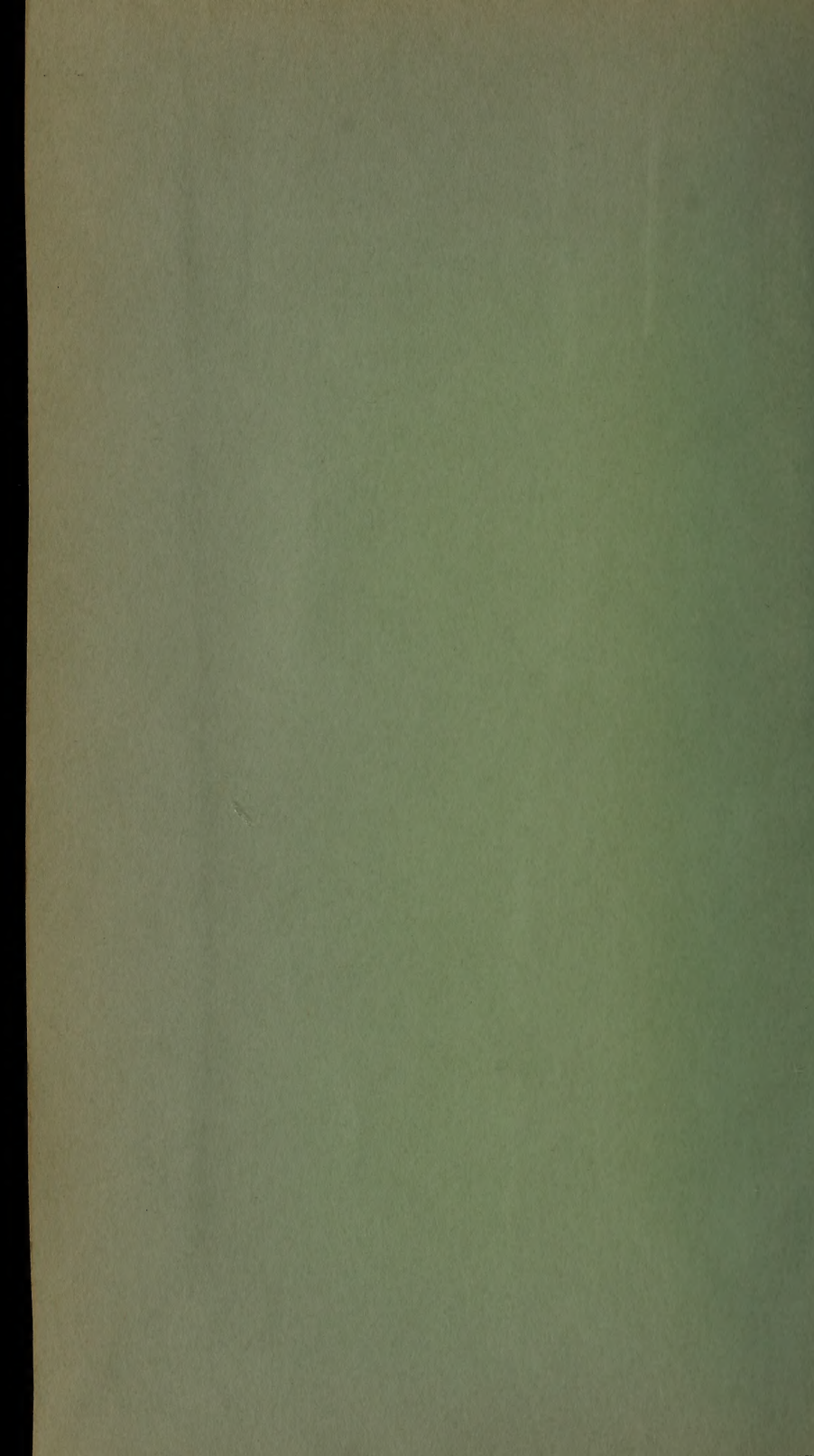
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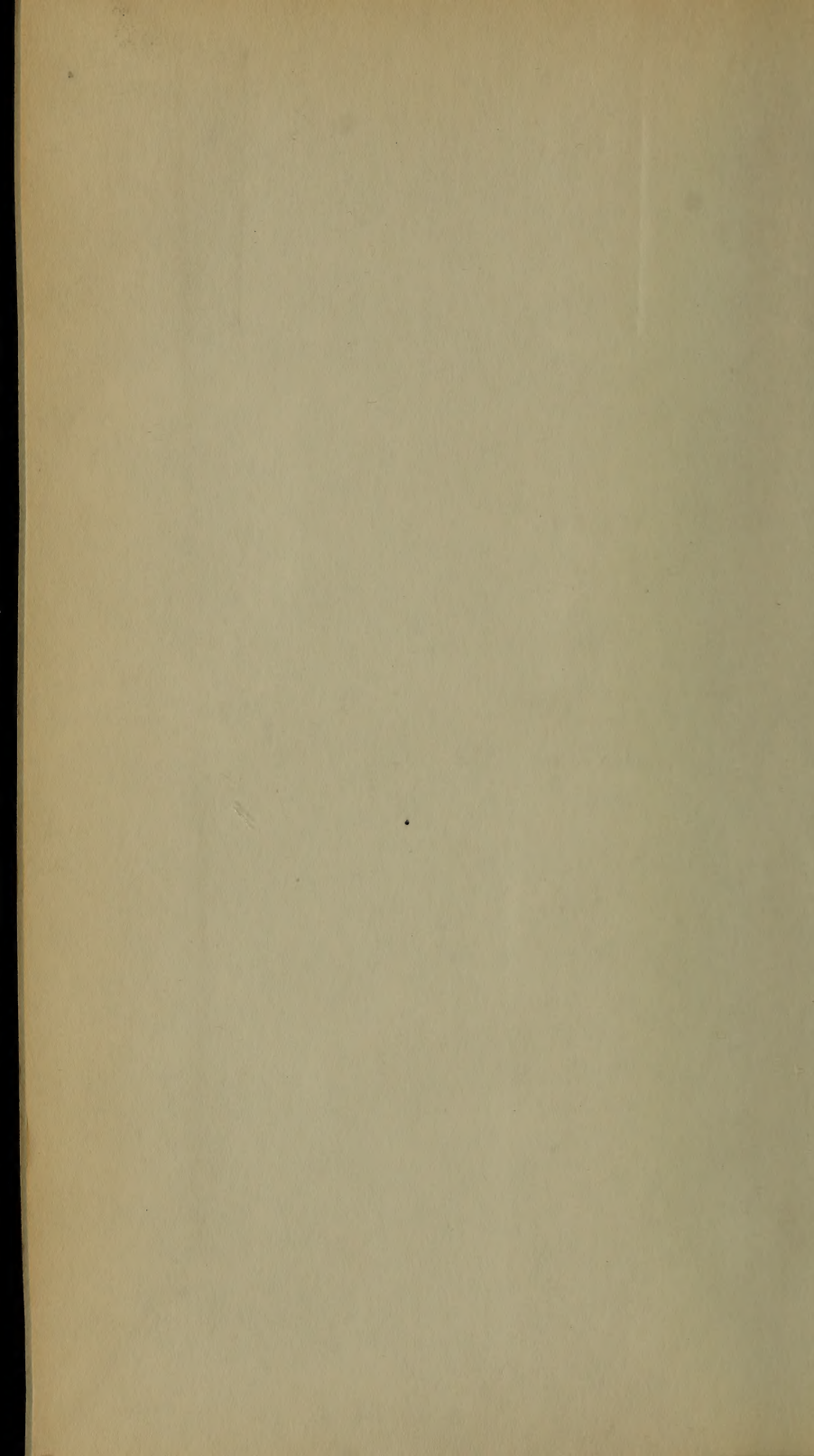














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LONDON, EDINBURGH, AND DUBLIN  
PHILOSOPHICAL MAGAZINE  
AND  
JOURNAL OF SCIENCE.

CONDUCTED BY

SIR ROBERT KANE, LL.D. F.R.S. M.R.I.A. F.C.S.

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AND

WILLIAM FRANCIS, PH.D. F.L.S. F.R.A.S. F.C.S.

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"Nec araneorum sane textus ideo melior quia ex se fila gignunt, nec noster  
vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit. lib. i. cap. 1. Not.*

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VOL. XLIII.—FOURTH SERIES.

JANUARY—JUNE 1872.

76041

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LONDON.

TAYLOR AND FRANCIS, RED LION COURT, FLEET STREET,

*Printers and Publishers to the University of London ;*

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"Meditationis est perscrutari occulta; contemplationis est admirari  
perspicua . . . . Admiratio generat quæstionem, quæstio investigationem,  
investigatio inventionem."—*Hugo de S. Victore.*

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—"Cur spirent venti, cur terra dehiscat,  
Cur mare turgescat, pelago cur tantus amaror,  
Cur caput obscura Phœbus ferrugine condât,  
Quid toties diros cogat flagrare cometas;  
Quid pariat nubes, veniant cur fulmina cœlo,  
Quo micet igne Iris, superos quis conciat orbes  
Tam vario motu."

*J. B. Pinelli ad Mazonium.*



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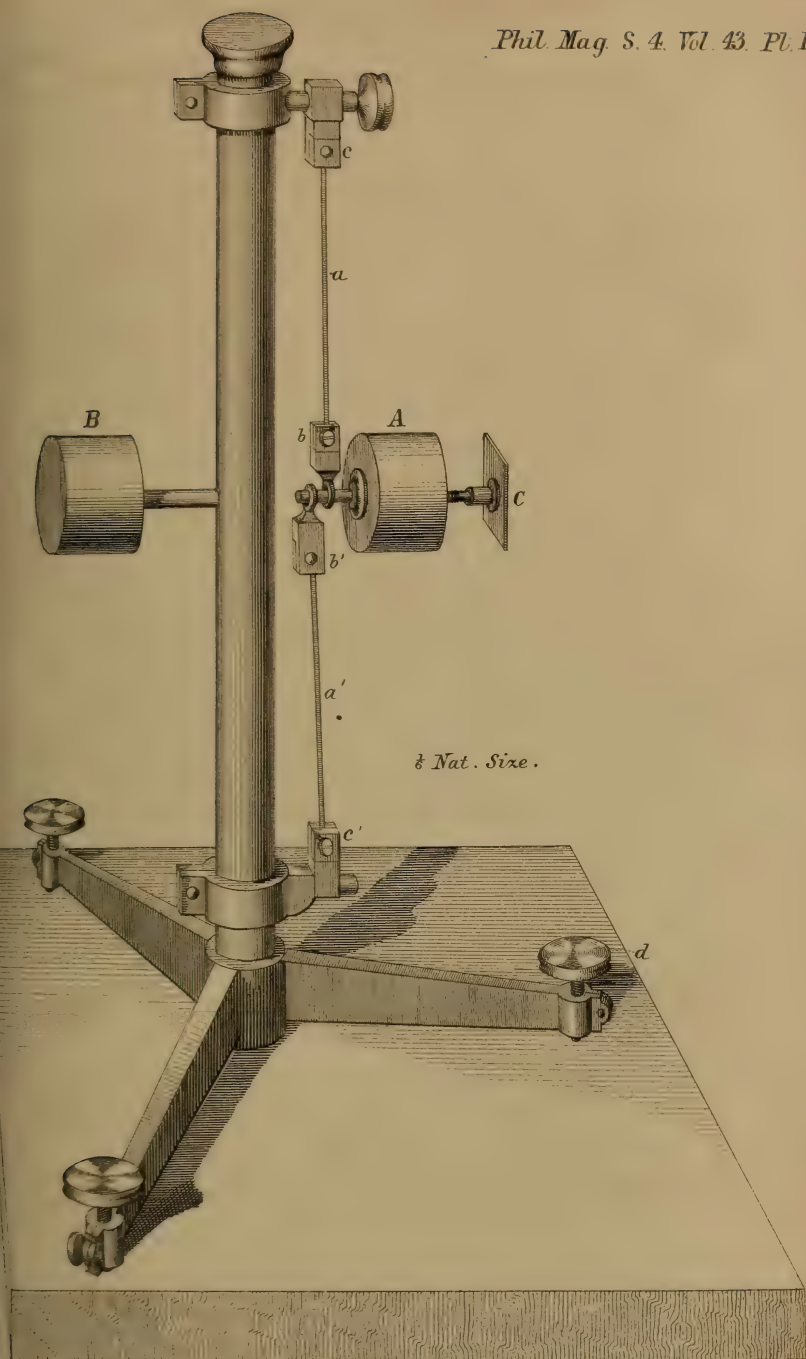
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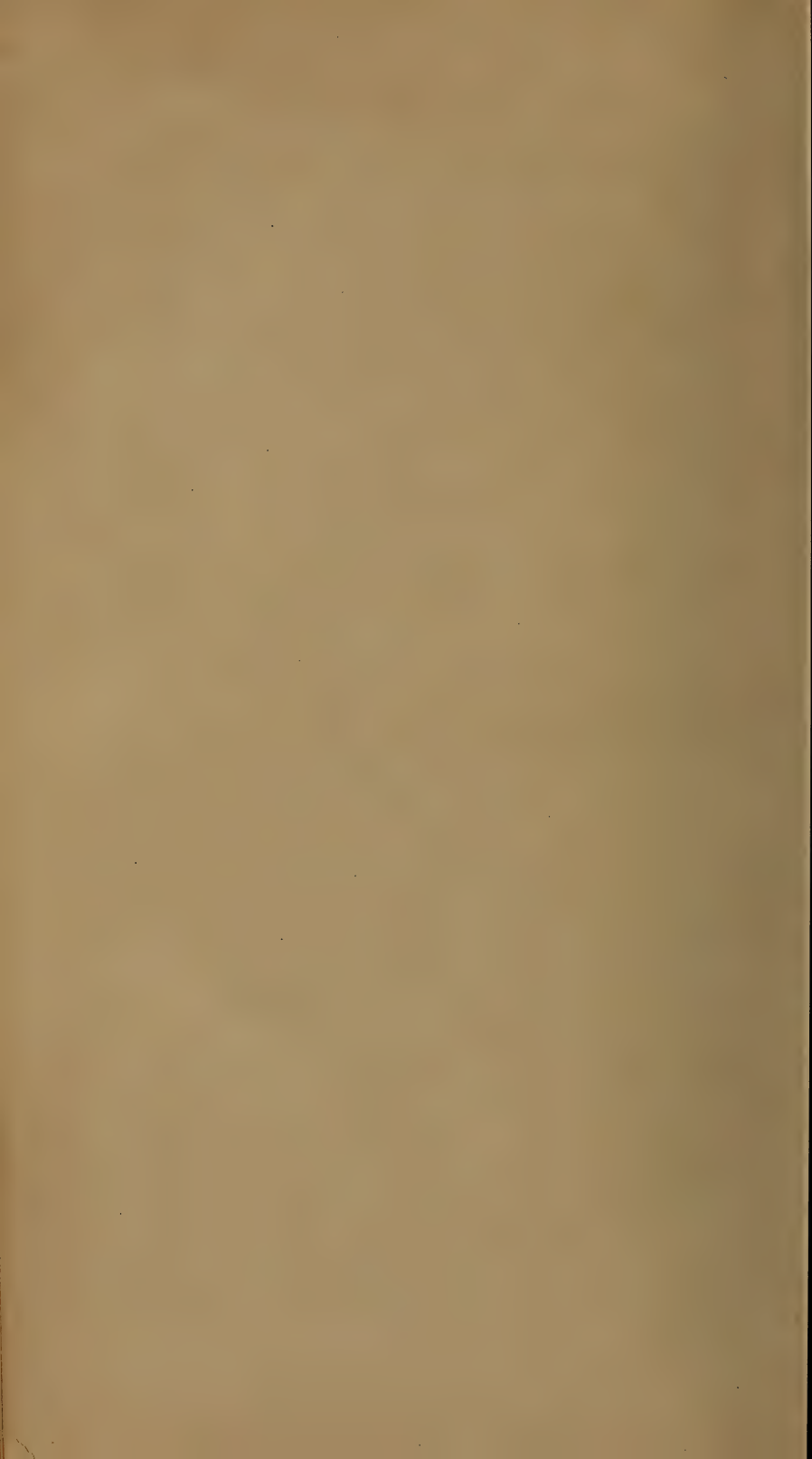
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- III. Illustrative of M. F. Zöllner's Paper on the Origin of the Earth's Magnetism, and the Magnetic Relations of the Heavenly Bodies.



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[FOURTH SERIES.]

JANUARY 1872.

I. *Electrodynamic Measurements.* By Professor WILHELM WEBER.—Sixth Memoir, relating specially to the Principle of the Conservation of Energy\*.

THE law of electrical action announced in the First Memoir on Electrodynamic Measurements (*Elektrodynamische Maassbestimmungen*, Leipzig, 1846) has been tested on various sides and been modified in many ways. It has also been made the subject of observations and speculations of a more general kind; these, however, cannot by any means be regarded as having as yet led to definite conclusions. The First Part of the following Memoir is limited to a discussion of the relation which this law bears to the *Principle of the Conservation of Energy*, the great importance and high significance of which have been brought specially into prominence in connexion with the Mechanical Theory of Heat. In consequence of its having been asserted that the law referred to is in contradiction with this principle, an endeavour is here made to show that no such contradiction exists. On the contrary, the law enables us to make an addition to the Principle of the Conservation of Energy, and to alter it so that its application to each pair of particles is no longer limited solely to the time during which the pair does not undergo either increase or diminution of *vis viva* through the action of other bodies, but always holds good independently of the manifold relations to other bodies into which the two particles can enter.

Besides this, in the Second Part the law is applied to the de-

\* Translated by Professor G. C. Foster, F.R.S., from the *Abhandlungen der mathem.-phys. Classe der Königl. Sächsischen Gesellschaft der Wissenschaften*, vol. x. (January 1871).

velopment of the equations of motion of two electrical particles subjected only to their mutual action. Albeit this development does not lead directly to any comparisons or exact control by reference to existing experience (on which account it has hitherto received little attention), it nevertheless leads to various results which appear to be of importance as furnishing clues for the investigation of the molecular conditions and motions of bodies which have acquired such special significance in relation to Chemistry and the theory of Heat, and to offer to further investigation interesting relations in these still obscure regions.

### ON THE RELATION BETWEEN THE LAWS OF ELECTRICITY AND THE PRINCIPLE OF THE CONSERVATION OF ENERGY.

#### 1. *Electrical Particles and Electrical Masses.*

Particles of positive and of negative electricity are denoted by the same letters, for instance by  $e$  or  $e'$  &c., but a positive or a negative value is assigned to  $e$  or  $e'$  ... according to whether it represents a particle of the positive or of the negative fluid.

If the measurable force of repulsion exerted by the first particle  $e$  upon another exactly equal particle  $e$  at the constant measurable distance  $r$  be denoted by  $f$ , and also the measurable force of repulsion exerted by the second particle  $e'$  upon another exactly equal particle  $e'$ , at the same distance  $r$ , be denoted by  $f'$ , then  $\pm r\sqrt{f}$  is taken as the measure of  $e$ , and  $\pm r\sqrt{f'}$  as the measure of  $e'$ , where the upper or the lower sign is to be taken according to whether the particle is a particle of positive or of negative fluid. The unit of force which is here adopted for the measurement of  $f$  and  $f'$  is the unit recognized in Mechanics, namely the force which, when it acts upon the unit of mass recognized in Mechanics (1 milligramme), imparts to this mass unit of velocity in unit of time. The repulsive force of the two particles  $e, e'$ , so long as their distance  $r$  remains unchanged, is, in accordance with the electrostatical law,

$$= \frac{ee'}{rr}.$$

A negative value of this expression denotes attractive force.

In this mode of denoting particles of the electric fluids, however,  $e, e'$  have not the signification of *masses* in the mechanical sense, as appears from the simple consideration that  $e, e'$  may have at one time positive and at another time negative values; but nevertheless the values of  $e, e'$  are closely related to the masses of the particles. For if we denote the *masses* of the particles  $e, e'$  (in the mechanical sense, according to which the unit of mass [1 milligramme] is determined by the mass of one ponderable body, and different masses are compared with each other

in proportion to the reciprocals of the accelerations produced in them by the same force) by  $\epsilon, \epsilon'$ , of which the values are always positive, we get for *positive* values of  $e, e'$ ,

$$\frac{e}{\epsilon} = \frac{e'}{\epsilon'} = a;$$

and for negative values of  $e, e'$ ,

$$\frac{e}{\epsilon} = \frac{e'}{\epsilon'} = b,$$

where  $a$  has a definite *positive* and  $b$  a definite *negative* value. Whether or not we have here  $aa=bb$ , or what ratio  $aa$  bears to  $bb$ , has not as yet been made out, any more than the numerical value of  $a$  or  $b$ . In many cases the electrical mass  $\epsilon$  is connected with a ponderable mass  $m$ , so that it is impossible for it to be moved independently of it; in such cases, only the combined mass  $m + \epsilon$  comes into account, and in general  $\epsilon$  may be regarded as vanishingly small in comparison with  $m$ . Consequently it is only seldom that the masses  $\epsilon, \epsilon'$  have to be considered.

The distinction here indicated between the particles  $e, e'$  and their masses  $\epsilon, \epsilon'$  is not always made; on the contrary, the symbols of the particles  $e, e'$  are also used to denote the corresponding masses. It is, however, to be observed that, when this is done, no regard can be had to the signs of  $e, e'$ . The omission of the unknown factors  $a$  and  $b$  is always allowable when we are dealing only with the *relative values* of masses of positive or of negative electricity.

## 2. The Law of Electrical Force.

The Law of Electrical Force is thus stated in 'Electrodynamic Measurements' (Leipzig, 1846, p. 119:—

If  $e$  and  $e'$  denote two electrical particles, the repulsive force exerted by the two particles on each other at the distance  $r$  is represented by

$$\frac{ee'}{rr} \left( 1 - \frac{1}{cc} \frac{dr^2}{dt^2} + \frac{2r}{cc} \frac{ddr}{dt^2} \right),$$

where  $c$  is the constant denoted at the place quoted by  $\frac{4}{a}$ .

But this expression for the force which the particles  $e$  and  $e'$  mutually exert upon each other, it is easy to see, is dependent on a magnitude which contains as a factor the very force that is to be determined. This is readily seen when the relative acceleration of the two particles, namely  $\frac{ddr}{dt^2}$ , is broken up into two parts, thus,

$$\frac{ddr}{dt^2} = \frac{ddr'}{dt^2} + \frac{ddr''}{dt^2};$$



where the first part,  $\frac{ddr'}{dt^2}$ , is the relative acceleration due to the mutual action of the two particles, and the second part,  $\frac{ddr''}{dt^2}$ , is the acceleration due to other causes (namely to the acquired velocity of the particles perpendicular to  $r$ , and to the mutual action between them and other bodies). The first part, however, or that due to the mutual action of the two particles, is proportional to the force arising from this mutual action, and is represented by the quotient of this force by the mass upon which it acts.

Hence there easily follows, as was shown in the memoir already quoted (page 168), another expression for the force which the particles  $e$  and  $e'$  mutually exert upon each other, containing only terms which are independent of the force to be determined, namely the expression

$$\frac{ee'}{rr - \frac{2r}{cc}(e+e')} \left( 1 - \frac{1}{cc} \frac{dr^2}{dt^2} + \frac{2rf}{cc} \right)$$

(in which  $f$  is put for  $\frac{ddr''}{dt^2}$ ), or, if the electrical particles  $e$  and  $e'$  are distinguished from their masses  $\epsilon$  and  $\epsilon'$  in accordance with the previous section (a distinction which was not made in the memoir quoted above), the expression

$$\frac{ee'}{rr - \frac{2r}{cc} \cdot \frac{\epsilon + \epsilon'}{\epsilon\epsilon'}} \left( 1 - \frac{1}{cc} \frac{dr^2}{dt^2} + \frac{2rf}{cc} \right).$$

From this it results that the law of electrical force is by no means so simple as we expect a fundamental law to be; on the contrary, it appears in two respects to be particularly complex.

In the first place, it is a consequence of this expression for the force, that, as was pointed out in the memoir referred to, the force which two electrical particles exert upon each other does not depend exclusively upon these particles themselves, but also upon the portion of their relative acceleration denoted by  $f$ , which is in part due to the action of other bodies. It was also pointed out that, inasmuch as the forces exerted by two bodies upon each other have been called by Berzelius *catalytic forces* when they depend upon the presence of a third body, electrical forces considered generally are, in this sense, catalytic forces.

In the second place, another noteworthy result follows from this expression for the force—namely, that when the particles  $e$  and  $e'$  are of the same kind, *they do not by any means always*

repel each other; thus when  $\frac{dr^2}{dt^2} < cc + 2rf$ , they repel only so long

as  $r > \frac{2}{cc} \frac{\epsilon + \epsilon'}{\epsilon\epsilon'} ee'$ , and, on the contrary, they attract when

$$r < \frac{2}{cc} \frac{\epsilon + \epsilon'}{\epsilon\epsilon'} ee'.$$

An exception to this rule occurs only in the case in which  $\left(r - 2 \frac{\epsilon + \epsilon'}{\epsilon\epsilon'} \frac{ee'}{cc}\right)$ , which is always a factor of the denominator, becomes likewise a factor of the numerator. This case occurs when the two electrical particles are at *permanent relative rest*, so that  $\frac{dr}{dt} = 0$  and  $\frac{ddr}{dt^2} = 0$ .

The general expression for the force given above becomes in fact

$$\frac{ee'}{r \left(r - 2 \frac{\epsilon + \epsilon'}{\epsilon\epsilon'} \frac{ee'}{cc}\right)} \cdot \left(1 + \frac{2r}{cc} f\right)$$

when  $\frac{dr}{dt} = 0$ ; and by dividing this by the mass  $\frac{\epsilon\epsilon'}{\epsilon + \epsilon'}$ , we find the part of the acceleration which is due to the forces exerted upon each other by the two electrical particles, namely

$$\frac{(\epsilon + \epsilon') ee'}{\epsilon\epsilon' r \left(r - 2 \frac{\epsilon + \epsilon'}{\epsilon\epsilon'} \cdot \frac{ee'}{cc}\right)} \cdot \left(1 + \frac{2r}{cc} f\right).$$

By adding to this the other part of the acceleration, namely  $f$ , which is due to the acquired motion of the particles at right angles to  $r$  and to the action of other bodies, we obtain the *total* acceleration, namely

$$\frac{ddr}{dt^2} = f + \frac{(\epsilon + \epsilon') ee'}{\epsilon\epsilon' r \left(r - 2 \frac{\epsilon + \epsilon'}{\epsilon\epsilon'} \cdot \frac{ee'}{cc}\right)} \cdot \left(1 + \frac{2r}{cc} f\right),$$

which, when the particles are at permanent relative rest,  $= 0$ . Hence for permanent relative rest we have

$$f = - \frac{\epsilon + \epsilon'}{\epsilon\epsilon'} \cdot \frac{ee'}{rr}.$$

If this value of  $f$  be substituted in the expression for the force

$$\frac{ee'}{r \left(r - 2 \frac{\epsilon + \epsilon'}{\epsilon\epsilon'} \cdot \frac{ee'}{cc}\right)} \cdot \left(1 + \frac{2r}{cc} f\right),$$

the latter becomes

$$\frac{ee'}{r\left(r - 2 \frac{\epsilon + \epsilon'}{\epsilon\epsilon'} \cdot \frac{ee'}{cc}\right)} \cdot \frac{1}{r} \left(r - 2 \frac{\epsilon + \epsilon'}{\epsilon\epsilon'} \cdot \frac{ee'}{cc}\right).$$

Hence it appears that, in the case of permanent relative rest, the factor  $\left(r - 2 \frac{\epsilon + \epsilon'}{\epsilon\epsilon'} \cdot \frac{ee'}{cc}\right)$  is common to numerator and denominator. The value of the quotient, which is thus independent of this factor, namely  $\frac{ee'}{rr}$ , consequently gives the expression for the force, in the case of permanent relative rest, in complete agreement with the fundamental laws of electrostatics, according to which this force has a *positive* value for particles of the *same kind* at *all distances*.

### 3. The Law of Electrical Potential.

In the previous section the law of electrical force is shown to be, in two respects, of a very complicated character, namely:—in the first place, in that the repulsive force between two electrical particles is dependent on things that do not appertain either to the nature of the particles which exert the force upon each other, or to their relative positions in space, or their existing relative motion, but *depends upon other bodies*; and secondly, in that *repulsion* may be exerted upon each other at certain distances by the same particles, and *attraction* at other distances.

Compared with this complicated law of *electrical force*, the law of *electrical potential* is very simple.

The value of the potential  $V$  of two electrical particles  $e$  and  $e'$ , in fact, as I pointed out as long ago as the year 1848 in Pogendorff's *Annalen* (vol. lxxiii. p. 229), is determined by the following law,

$$V = \frac{ee'}{r} \left( \frac{1}{cc} \cdot \frac{dr^2}{dt^2} - 1 \right).$$

Observing that both  $r$  and  $\frac{dr}{dt}$  have different values at different times for both the particles  $e$  and  $e'$ , and that consequently both are functions of the time, it follows that  $\frac{dr}{dt}$  may also be regarded as a function of  $r$ , which may be denoted by  $fr$ . We thus obtain

$$V = \frac{ee'}{r} \left( \frac{1}{cc} \cdot (fr)^2 - 1 \right),$$



and from this, by differentiation, the expression for the force

$$\frac{dV}{dr} = -\frac{ee'}{rr} \left( \frac{1}{cc} \cdot (fr)^2 - 1 \right) + 2 \frac{ee'}{rcc} \cdot fr \cdot \frac{dfr}{dr},$$

or, if we again put  $\frac{dr}{dt}$  for  $fr$ ,

$$\frac{dV}{dr} = \frac{ee'}{rr} \left( 1 - \frac{1}{cc} \cdot \frac{dr^2}{dt^2} + \frac{2r}{cc} \cdot \frac{dr}{dt} \cdot \frac{d}{dr} \frac{dr}{dt} \right),$$

for which we may write

$$\frac{dV}{dr} = \frac{ee'}{rr} \left( 1 - \frac{1}{cc} \frac{dr^2}{dt^2} + \frac{2r}{cc} \cdot \frac{ddr}{dt^2} \right).$$

From this it appears that

$$\frac{ee'}{r} \left( \frac{1}{cc} \cdot \frac{dr^2}{dt^2} - 1 \right)$$

is a function whose differential coefficient with respect to  $r$  represents the repulsive force between the two particles  $e$  and  $e'$ ,

where  $r$  and  $\frac{dr}{dt}$  denote respectively their distance and relative velocity regarded as functions of the time. But since  $\frac{ee'}{r} \left( \frac{1}{cc} \cdot \frac{dr^2}{dt^2} - 1 \right)$

becomes equal to nothing when  $e$  and  $e'$  are separated infinitely far from each other,  $\frac{ee'}{r} \left( \frac{1}{cc} \cdot \frac{dr^2}{dt^2} - 1 \right)$  is the *potential* of the electrical particles  $e$  and  $e'$ —that is to say, the *work* which is expended in causing the particles to approach each other from an infinite distance while under the action of their mutual repulsion,

and to arrive at the distance  $r$  with the relative velocity  $\frac{dr}{dt}$  \*.

It likewise results from the foregoing that the *work*, which is expended when a given relative arrangement and state of motion of a system of particles  $e, e'$  are changed to another arrangement and another state of motion, depends only on the initial and

\* This law of electrical *potential* has also been taken as his starting-point by Beer in his 'Introduction to Electrodynamics' (see *Einleitung in die Elektrostatik, die Lehre vom Magnetismus und die Elektrodynamik*, von August Beer. Nach dem Tode des Verfassers herausgegeben von Julius Plücker: Braunschweig, 1865. S. 250). The placing of the law of *potential* in the foreground as the fundamental law, and deriving the law of force from it, ought not to give rise to any misgiving. We have in many respects a better justification for speaking of the *physical existence of the work expressed by the potential* than for speaking of the *physical existence of a force*, as to which all we can say is that it tends to change the *physical relations of bodies*.

final arrangements and movements of the particles, and is independent of the way by which the transition has been effected, and also independent of states of motion which may have existed during the transition.

#### 4. *Fundamental Electrical Laws.*

The law of *electrical potential* certainly appears to stand, in view of its simplicity, in a much closer relation to the true fundamental laws of electricity than the far more complex law of *electrical force*; but the expression of the former law may still be resolved into two simpler laws, which may be stated in the following manner:—

*First Law.*—If two particles  $e$  and  $e'$  are at relative rest or possess the same relative motion at two different distances  $r$  and  $\rho$ , the quantities of work  $V$  and  $U$  which are expended in separating the particles, while mutually acting on each other, from these distances to an infinite distance, are to each other inversely as these two distances, that is,

$$V : U = \rho : r. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

*Second Law.*—The work  $U$ , which is expended in separating the particles  $e$  and  $e'$  while subject to the force exerted by them on each other from a given distance  $\rho$  ( $= \frac{ee'}{a}$ ) proportional to the quantity  $ee'$  to an infinite distance, makes together with the *vis viva*  $x$ , which belonged to the particles in consequence of their relative motion at the distance  $\rho$ , a constant sum, namely  $a$ ; that is,

$$U + x = a. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

For from equation (1) it follows that

$$U = \frac{r}{\rho} V;$$

and hence, by equation (2),

$$\frac{r}{\rho} V + x = a,$$

or, since  $\rho = \frac{ee'}{a}$ ,

$$V = \frac{ee'}{r} \left( 1 - \frac{x}{a} \right).$$

But the relative *vis viva*  $x$  is proportional to the square of the relative velocity  $\frac{dr}{dt}$ , so that we may substitute for  $a$  a new con-

stant  $cc$ , such that

$$\frac{x}{a} = \frac{1}{cc} \cdot \frac{dr^2}{dt^2}^*.$$

\* If  $\epsilon$  and  $\epsilon'$  denote the masses of the particles  $e$  and  $e'$ ,  $\alpha$  and  $\beta$  the velocities of  $\epsilon$  in the direction of  $r$  and at right angles thereto, and  $\alpha'$  and  $\beta'$  the same velocities for  $\epsilon'$ , so that  $\alpha - \alpha' = \frac{dr}{dt}$  is the relative velocity of the two particles, then

$$\frac{1}{2} \epsilon(\alpha\alpha + \beta\beta) + \frac{1}{2} \epsilon'(\alpha'\alpha' + \beta'\beta')$$

is the total *vis viva* of the two particles. If we now put for  $\alpha$

$$\frac{\epsilon\alpha + \epsilon'\alpha'}{\epsilon + \epsilon'} + \frac{\epsilon'(\alpha - \alpha')}{\epsilon + \epsilon'},$$

and for  $\alpha'$

$$\frac{\epsilon\alpha + \epsilon'\alpha'}{\epsilon + \epsilon'} - \frac{\epsilon'(\alpha - \alpha')}{\epsilon + \epsilon'},$$

we get the total *vis viva* of the two particles represented as the sum of two parts in the following manner—namely,

$$= \frac{1}{2} \frac{\epsilon\epsilon'}{\epsilon + \epsilon'} \cdot \frac{dr^2}{dt^2} + \frac{1}{2} \left[ \frac{(\epsilon\alpha + \epsilon'\alpha')^2}{\epsilon + \epsilon'} + \epsilon\beta\beta + \epsilon'\beta'\beta' \right],$$

the *first* part of which, or  $\frac{1}{2} \frac{\epsilon\epsilon'}{\epsilon + \epsilon'} \cdot \frac{dr^2}{dt^2}$ , is the *relative vis viva* of the particles which was denoted above by  $x$ . But  $a$  is also a relative *vis viva* of the same particles, namely that which corresponds to a definite relative velocity  $c$ , so that  $a = \frac{1}{2} \frac{\epsilon\epsilon'}{\epsilon + \epsilon'} \cdot cc$ . Hence we get  $\frac{x}{a} = \frac{1}{cc} \cdot \frac{dr^2}{dt^2}$ , as was given above.

It may be further observed that the *second* part of the above sum, namely  $\frac{1}{2} \left[ \frac{(\epsilon\alpha + \epsilon'\alpha')^2}{\epsilon + \epsilon'} + \epsilon\beta\beta + \epsilon'\beta'\beta' \right]$ , may be again represented, after another subdivision, as the sum of two parts, thus

$$= \frac{1}{2} \frac{\epsilon\epsilon'}{\epsilon + \epsilon'} \cdot \frac{ds^2}{dt^2} + \frac{1}{2} \left[ \frac{(\epsilon\alpha + \epsilon'\alpha')^2}{\epsilon + \epsilon'} + (\epsilon + \epsilon')\gamma\gamma \right],$$

where  $\frac{ds}{dt}$  represents the velocity with which the two particles move relatively to each other in space perpendicularly to  $r$ , while  $\gamma$  represents the velocity, perpendicular to  $r$ , of the centre of gravity of the two particles. We thus get the total *vis viva* of the two particles divided into three parts—namely,

- i.  $\frac{1}{2} \frac{\epsilon\epsilon'}{\epsilon + \epsilon'} \cdot \frac{dr^2}{dt^2}$ ,
- ii.  $\frac{1}{2} \frac{\epsilon\epsilon'}{\epsilon + \epsilon'} \cdot \frac{ds^2}{dt^2}$ ,
- iii.  $\frac{1}{2} \left[ \frac{(\epsilon\alpha + \epsilon'\alpha')^2}{\epsilon + \epsilon'} + (\epsilon + \epsilon')\gamma\gamma \right]$ ;

the *first* of which, namely  $\frac{1}{2} \frac{\epsilon\epsilon'}{\epsilon + \epsilon'} \cdot \frac{dr^2}{dt^2}$ , represents the *relative vis viva* of



We thus obtain

$$V = \frac{ee'}{r} \left( 1 - \frac{1}{cc} \cdot \frac{dr^2}{dt^2} \right).$$

Here  $V$  denotes the work expended in separating the two particles from the distance  $r$  to an infinite distance. If  $V$  is to denote the work done in bringing the particles from an infinite distance to the distance  $r$ , as it is usually understood to do, so that positive values of  $\frac{dV}{dr}$  may indicate *repulsion*, we obtain

$$V = \frac{ee'}{r} \left( \frac{1}{cc} \cdot \frac{dr^2}{dt^2} - 1 \right);$$

that is to say, the *law of electrical potential*.

### 5. *Principle of the Conservation of Energy for Two Particles which form a detached system.*

The two fundamental laws laid down in the foregoing section, which may be called

The Law of the dependence of the Potential on the distance  
for a *constant relative motion*, and

The Law of the dependence of the Potential on the relative  
motion for a *constant distance*,

require to be further discussed in relation to their bearing upon the principle of the Conservation of Energy.

In accordance with the principle of the conservation of energy, three forms of energy are to be distinguished from each other—namely, *energy of motion* (kinetic energy), *potential energy*, and *energy of heat* (thermal energy).

The *energy of motion* is that part of the energy which depends upon the existing movements; and a special determination is given of the way in which it depends upon movement—namely, partly upon the magnitude of the moving mass, and partly upon the velocity with which this mass moves.

The same determination also applies to *thermal energy*, if this is regarded, in accordance with the mechanical theory of heat,

the two particles; while the *first* two parts taken together, namely

$$\frac{1}{2} \frac{\epsilon\epsilon'}{\epsilon + \epsilon'} \left( \frac{dr^2}{dt^2} + \frac{ds^2}{dt^2} \right),$$

represent the total *internal vis viva*, or the total *internal kinetic energy of the system*; and the *third* part, namely  $\frac{1}{2} \left[ \frac{(\epsilon\alpha + \epsilon'\alpha')^2}{\epsilon + \epsilon'} + (\epsilon + \epsilon')\gamma\gamma \right]$ , repre-

sents the *external vis viva*, or the *external kinetic energy of the system* (that is, the *vis viva* of the centre of gravity of the two particles).

as an *internal motion of the particles of bodies*. But if we are dealing with a system of two *elementary particles* (that is to say, particles such that there can be no motion *within* them), it is obvious that in the case of such a system thermal energy has no existence, and *energy of motion* and *potential energy* alone remain.

Lastly, the *potential energy* is that part of the energy which depends on the existing potential; and a special determination is needed of the way in which potential energy *depends upon the potential*, exactly as, in the case of the energy of motion, it is needful to determine the special way in which it depends on movement.

Now this special determination has been made by *equating potential energy* (without regard to the sign) and *potential\**.

The justification for this proceeding has been found in the fact that the potential is a magnitude which is homogeneous with kinetic energy, which, when taken with the negative sign and added to the kinetic energy, gives always the same sum, so long as the two particles constitute a detached system which does not undergo either gain or loss of energy from without.

For instance, if we have a system of two ponderable particles  $m, m'$ , its *potential* is

$$V = \frac{mm'}{r};$$

and the internal *vis viva*, or the *internal kinetic energy of the system*, is

$$W = \frac{1}{2} \frac{mm'}{m+m'} (uu + \alpha\alpha),$$

where  $u = \frac{dr}{dt}$  is the relative velocity of the two particles, and  $\alpha$  the difference of the velocities in space perpendicularly to  $r$ . But, for such a *detached system*, if we put  $r=r_0$  and  $\alpha=\alpha_0$

\* The sign of the *potential*,  $V$ , is so determined that positive values of  $\frac{dV}{dr}$  indicate repelling forces; the sign of the *potential energy* is fixed by the sign of the work which is done, in consequence of the mutual action of the particles, when the two particles are separated from the distance  $r$  to an infinite distance. Consequently, for two ponderable particles  $m, m'$ , the potential is  $V = \frac{mm'}{r}$ , and the potential energy  $= -\frac{mm'}{r}$ . For two electrical particles  $e, e'$  the potential is  $= \frac{ee'}{r} \left( \frac{1}{cc} \cdot \frac{dr^2}{dt^2} - 1 \right)$ , and the potential energy  $= \frac{ee'}{r} \left( 1 - \frac{1}{cc} \cdot \frac{dr^2}{dt^2} \right)$ .

when  $u=0$ , the following value is easily got, namely

$$uu = \frac{r_0 - r}{r_0} \left[ \frac{2(m+m')}{r} - \frac{r_0 + r}{r_0} \alpha \alpha \right]^*,$$

and consequently the sum

$$W - V = - \frac{mm'}{r_0} + \frac{1}{2} \frac{mm'}{m+m'} \cdot \alpha_0 \alpha_0.$$

This sum always retains the same value as long as the values of  $r_0$  and  $\alpha_0$  remain unchanged—that is, so long as the system of the two particles undergoes neither loss nor gain of energy from without. The *external kinetic energy* of such a detached system amounts *separately to a constant sum*.

Now the same thing holds good also for two *electrical* particles  $e, e'$ ; for their potential, taken with the negative sign and added to their kinetic energy, gives in like manner always the same sum so long as the particles constitute a *detached* system.

\* The force with which the two particles mutually act on each other, namely  $\frac{dV}{dr}$ , divided by  $m$ , gives the acceleration of the particle  $m$ —that is,

$\frac{1}{m} \cdot \frac{dV}{dr}$ ; divided by  $m'$  it gives the acceleration of the particle  $m'$ , namely

$\frac{1}{m'} \cdot \frac{dV}{dr}$ . Consequently that part of the relative acceleration of the two

particles which arises from their mutual action is  $\left(\frac{1}{m} + \frac{1}{m'}\right) \frac{dV}{dr}$ , while that part of the relative acceleration of the two particles which arises from their rotation about one another is represented by  $\frac{\alpha\alpha}{r}$ . If now this last

portion be subtracted from the total acceleration  $\frac{du}{dt}$ , the following equation results:

$$\frac{du}{dt} - \frac{\alpha\alpha}{r} = \left(\frac{1}{m} + \frac{1}{m'}\right) \frac{dV}{dr}.$$

Putting  $r=r_0$  and  $\alpha=\alpha_0$  for the instant at which  $u=0$ , we obtain the expression

$$\alpha r = \alpha_0 r_0$$

as applicable for the case in which the only forces acting on the two particles are those due to their mutual action. Accordingly we get, by integrating the above differential equation after it has been multiplied by  $2dr=2udt$ ,

$$uu + \alpha_0 \alpha_0 r_0 r_0 \left( \frac{1}{rr} - \frac{1}{r_0 r_0} \right) = 2 \left( \frac{1}{m} + \frac{1}{m'} \right) \left( \frac{mm'}{r} - \frac{mm'}{r_0} \right),$$

and hence

$$uu = \frac{r_0 - r}{r} \left( \frac{2(m+m')}{r_0} - \frac{r_0 + r}{r} \alpha_0 \alpha_0 \right) = \frac{r_0 - r}{r_0} \left( \frac{2(m+m')}{r} - \frac{r_0 + r}{r_0} \alpha \alpha \right).$$



We have, for the *potential* of such a system of two electrical particles,

$$V = \frac{ee'}{r} \left( \frac{uu}{cc} - 1 \right),$$

and, for the *internal kinetic energy* of the system,

$$W = \frac{1}{2} \frac{\epsilon\epsilon'}{\epsilon + \epsilon'} (uu + \alpha\alpha) = \frac{ee}{\rho cc} (uu + \alpha\alpha),$$

if  $u = \frac{dr}{dt}$  denotes the relative velocity of the two particles, and  $\alpha$  the difference of their velocities in space at right angles to  $r$ . But, for such a *detached* system, when we put  $r = r_0$  and  $\alpha = \alpha_0$  for  $u = 0$ , it is easy to obtain

$$\alpha = \frac{r_0}{r} \alpha_0,$$

$$uu = \frac{r - r_0}{r - \rho} \left( \frac{\rho}{r_0} cc + \frac{r_0 + r}{r} \alpha_0 \alpha_0 \right)^*,$$

and consequently the sum

$$W - V = \frac{ee'}{r_0} + \frac{ee'}{\rho} \cdot \frac{\alpha_0 \alpha_0}{cc} = \frac{ee'}{r_0} + \frac{1}{2} \frac{\epsilon\epsilon'}{\epsilon + \epsilon'} \cdot \alpha_0 \alpha_0.$$

This sum likewise retains the same value so long as the values of  $r_0$  and  $\alpha_0$  remain unchanged—that is, so long as the system of two particles undergoes neither loss nor gain of energy from without†. *The same principle holds good in relation to the external kinetic energy of a detached system of two electrical particles and to that of two ponderable particles.*

\* See Section 11.

† In Professor Tait's very instructive work, 'A Sketch of Thermodynamics' (Edinburgh, 1868), the following passage occurs at page 76, in reference to the investigations of Riemann and Lorenz which appeared in Pogendorff's *Annalen* for 1867 [Phil. Mag. S. 4. vol. xxxiv. pp. 368 and 287]:—"But the investigations of these authors are entirely based on Weber's inadmissible theory of the forces exerted on each other by moving electric particles, for which the conservation of energy is not true, while Maxwell's result is in perfect consistence with that great principle." This assertion of Professor Tait's seems to be in contradiction with the above. At page 56 of the same work Mr. Tait mentions that Helmholtz has based the doctrine of energy on Newton's principle and on the following postulate:—"Matter consists of ultimate particles which exert upon each other forces whose directions are those of the lines joining each pair of particles, and whose magnitudes depend solely on the distances between the particles." The contradiction between the fundamental law of electricity and this postulate is evident; but the contradiction between it and the principle of the conservation of energy is by no means evident,—a distinction which Professor Tait seems to have overlooked.

6. *Extension of the Principle of the Conservation of Energy to two electrical particles which do not form a detached system.*

If potential energy is taken, as is done in the previous section, as equal and opposite to potential, the principle of the conservation of energy holds good for two particles only so long as these two particles constitute a *detached* system—that is, so long as the system formed of the two particles undergoes neither gain nor loss of energy from without.

If the *total* energy of such a detached system of two particles were at first  $=A$ , but, the system ceasing to be detached, it received from without a quantity of kinetic energy  $=a$ , it seems to follow that, if the system were now again to become detached, the *total* energy would again become and remain constant so long as it remained detached, but that the total energy of the system in its final detached state would have the value  $A+a$  (that is, a value exceeding that corresponding to its previous detached state by  $a$ ). This, however, does not by any means conclusively prove the impossibility of extending the principle of the conservation of energy to two electrical particles which do not constitute a detached system.

For, strictly speaking, this has only been proved on the assumption that the *potential energy* of the system depends solely on the *distance* between the two particles; while if, on the other hand, the potential energy does not depend simply on the distance of the two particles, but also on their relative *motion*, it is evident that while the system receives from without an amount of *kinetic energy*  $=a$ , a change in its *potential energy* may be indirectly produced thereby. It is thus possible that the change of *potential energy*, so caused indirectly from without, might be  $=-a$ , so that the *total* energy (kinetic energy and potential energy together) of the two particles, even if they did not constitute a detached system, would retain always the same value.

This, however, certainly does not occur in reality for a system of two electrical particles, if the *potential energy* is taken as *equal and opposite to the potential*; but this assumption, which would thus make the extension of the principle impossible, has by no means been proved to be a necessary one. In general, all that is required is a *special determination of the way in which the potential energy depends upon the potential*; and here all that is self-evident is, that inasmuch as potential and potential energy are homogeneous magnitudes, a purely numerical relation must exist between them. But whether this numerical relation is always that of  $+1$  to  $-1$ , or whether it is to be fixed otherwise, must still be regarded as in general doubtful; so that the possibility of the extension of the principle still remains.

We understand, in fact, by the *potential* of two particles, the amount of *work* which, in consequence of the mutual action of the two particles, is done when they are transferred in any way whatever from an infinite distance to the actually existing distance  $r$  with the actually existing relative velocity  $\frac{dr}{dt}$ .

It is, however, evident that *work* is done, in consequence of the mutual action of the two particles, not only during their transference from a *greater* distance to the distance  $r$ , but also during their transference from a *smaller* distance to the distance  $r$ . And there is no obvious reason why the *energy ascribed to the system* should be made to depend on the work done in the *former* case, and not on that done in the *latter* case also.

For example, if the *first* quantity of work were denoted, according to Section 4, by  $V$ , and the *second* by  $\frac{\rho-r}{\rho}V$ , the potential energy ascribed to the system might be taken as the *difference of these two amounts of work*, namely  $=\frac{\rho-r}{\rho}V - V = -\frac{r}{\rho}V$ .

This difference of the two amounts of work is evidently the quantity of work which is done, in consequence of the mutual action of the two particles, during their transference from the limiting value of *small* distances to the limiting value of *great* distances—that is to say, the value which  $-V = \frac{ee'}{r} \left(1 - \frac{uu}{cc}\right)$

assumes when  $r$  is taken therein as equal to the limiting value of *small* distances, or when we put  $r=\rho$ , where  $\rho$  denotes the limiting value of small distances. According to this, therefore, this *difference of the two quantities of work*  $= \frac{ee'}{\rho} \left(1 - \frac{uu}{cc}\right) = -\frac{r}{\rho}V$ .

In order to determine in this way the potential energy of a system of two electrical particles when the *first* quantity of work above referred to is

$$V = \frac{ee'}{r} \left( \frac{uu}{cc} - 1 \right),$$

it is only necessary further, for the determination of the *second* quantity of work, to determine the value of  $\rho$ —that is, of the *smaller distance* which is to be taken account of in that portion of the work:

Now this *smaller distance*, equally with the *greater distance*, must be determined *on its own account, independently of the actually existing conditions* of the two particles. This was done in the case of the *greater distance* by assigning to it an infinitely great value; in the case of the *smaller distance* the same thing



is accomplished if we assign to it the value  $2 \frac{\epsilon + \epsilon'}{\epsilon \epsilon} \cdot \frac{ee'}{cc}$ , a distance which is given by the particles  $e, e'$ , by their masses  $\epsilon, \epsilon'$ , and by the known electrical constant  $c$ .

If we now put the smaller distance equal to the value of  $\rho$ , we get, in virtue of the equations

$$V = \frac{ee'}{r} \left( \frac{uu}{cc} - 1 \right),$$

$$\frac{\rho - r}{\rho} V = \frac{\rho - r}{\rho} \cdot \frac{ee'}{r} \left( \frac{uu}{cc} - 1 \right),$$

the required value of the *potential energy*, namely

$$-\frac{r}{\rho} V = -\frac{ee'}{\rho} \left( \frac{uu}{cc} - 1 \right) = \frac{1}{2} \frac{\epsilon \epsilon'}{\epsilon + \epsilon'} (cc - uu).$$

In accordance with the distinction which is here drawn between the *potential* and the *potential energy* of two electrical particles and with the corresponding determination of their relation to each other, an analogous distinction may also be made between the *vis viva* and the *kinetic energy* of two particles. For there is no necessity that the *kinetic energy* of two particles should be taken as being equal to the *total vis viva of the two particles*; all that is generally essential is a *definite determination of the relation subsisting between the kinetic energy of two particles and the total vis viva belonging to them both*.

Now the total *vis viva* possessed by the two particles was represented in the note to section 4 as the sum of two parts, of which the *first* part, namely  $\frac{1}{2} \frac{\epsilon \epsilon'}{\epsilon + \epsilon'} \cdot \frac{dr^2}{dt^2}$ , was called the *relative vis viva*. The *second* part was that which the two particles possessed in virtue of their revolution about each other in space, and in virtue of the motion of their centre of gravity in space.

If now, in order to establish the conception of the *energy* of two particles, we take it as our starting-point that the *principle of the conservation of energy* of two particles must be based upon the essential characters of the two particles, and in fact upon what is *essential to them when regarded as constituting a detached system*, it is obvious that for this purpose the conception of the *energy* of two particles must be made to depend only on the relations presented by the system of the two particles as such, quite irrespectively of the relations in which these particles may stand to all other bodies in space.

Applying this fundamental principle to the *kinetic energy* of two particles in the same way as it has just been done in respect of the *potential energy*, we see that the *kinetic energy* must be taken as dependent upon the *first* part of the total *vis viva* be-

longing to the two particles—that is to say, upon their *relative vis viva*—and not upon the *second* part of the total *vis viva*, or that which the two particles possess in virtue of their revolution about one another in space or of the motion of their centre of gravity in space; for this latter part depends upon relations which the two particles do not of themselves directly present. For the two particles taken by themselves do not directly present any relation to space except their distance apart, from which no knowledge can be had of their rotation or of the motion of their centre of gravity in space.

Consequently, in what follows, by the *kinetic energy* of two particles is to be understood, not the total *vis viva* possessed by the two particles, but only their *relative vis viva*.

But it is easy to see that, in accordance with this, while a system of two electrical particles  $e, e'$  receives from without an amount of kinetic energy  $=a$ , it really undergoes an alteration of its *potential energy*  $=-a$ ; so that the *whole* energy of the system must always retain the same value not only when the two particles constitute a detached system, but also when they do not do so. For if we represent the *kinetic energy* communicated from without by

$$a = \frac{1}{2} \frac{\epsilon \epsilon'}{\epsilon + \epsilon'} vv,$$

while the kinetic energy of the particles *before* the communication of this portion was

$$= \frac{1}{2} \frac{\epsilon \epsilon'}{\epsilon + \epsilon'} \cdot u_0 u_0,$$

the kinetic energy existing *after* the communication is

$$\frac{1}{2} \frac{\epsilon \epsilon'}{\epsilon + \epsilon'} \cdot uu = \frac{1}{2} \frac{\epsilon \epsilon'}{\epsilon + \epsilon'} (u_0 u_0 + vv).$$

Consequently the *potential energy before the communication* is

$$-\frac{r}{\rho} V = \frac{1}{2} \frac{\epsilon \epsilon'}{\epsilon + \epsilon'} (cc - u_0 u_0),$$

whereas the *potential energy after the communication* is

$$-\frac{r}{\rho} V = \frac{1}{2} \frac{\epsilon \epsilon'}{\epsilon + \epsilon'} (cc - uu) = \frac{1}{2} \frac{\epsilon \epsilon'}{\epsilon + \epsilon'} (cc - u_0 u_0) - \frac{1}{2} \frac{\epsilon \epsilon'}{\epsilon + \epsilon'} vv;$$

so that, in consequence of the communication from without of *kinetic energy* equal to  $+a$ , a change of *potential energy* has occurred which is represented by

$$-\frac{1}{2} \frac{\epsilon \epsilon'}{\epsilon + \epsilon'} vv = -a.$$

7. *Application to other Bodies.*

If we distinguish, in accordance with the last section, between the potential and the potential energy of two particles—that is to say, if we define

*Potential* as the amount of *work* which, in consequence of the mutual action of the two particles, is done during the transference of the particles from an infinite distance to the actual distance  $r$  with the existing relative velocity  $\frac{dr}{dt}$ ; and

*Potential energy* as that amount of *work*, taken *negatively*, which, in consequence of the mutual action of the two particles, is done during the transference of the particles *from the greater distance*  $r = \infty$  *to the smaller distance*  $r = \rho$  determined by the particles  $e, e'$ , their masses  $\epsilon, \epsilon'$ , and by the constant  $c$ , with the existing relative velocity  $\frac{dr}{dt}$ ,—

the latter (that is to say, the *potential energy in the sense that has been indicated*) may be resolved into two parts, one of them equal and opposite to the *potential*, and therefore identical with the magnitude which has *hitherto* been alone called *potential energy*, but which, regarded henceforward as only a part of the potential energy, we may call the *free potential energy*; the remainder is the *second* part, which may be called the *latent potential energy*.

Hence the principle of the conservation of energy may be enunciated in the first place in the *earlier* wider sense as follows:—

For a *detached* system of two particles the sum of the *kinetic energy* and of the *free potential energy* is always the same.

For so long as no kinetic energy is either lost or communicated from without, every change in the free potential energy will be compensated by an equal and opposite change in the kinetic energy.

But the principle of the conservation of energy may also be enunciated, secondly, in the *narrower* sense as follows (potential energy and kinetic energy being understood in the sense that has just been defined):—

The *relative kinetic energy* of two particles, and the *total potential energy* which they possess along with this kinetic energy, together give always the same sum.

Upon this the following remarks may be made:—

(1) One particle regarded by itself can only possess *kinetic energy*.

(2) Two particles likewise possess in the first place kinetic energy, which is the sum of those which they possess when considered separately,



(3) This sum consists of a part A, which may be ascribed partly to the motion of their centre of gravity, and partly to their rotation about one another in space—and of another part B, which the particles possess relatively to each other when considered by themselves. This latter part, B, is called the *relative kinetic energy*, or *that belonging to the system formed by the two particles*.

(4) But in the *system of two particles* there is a something, in addition to its kinetic energy, which does not belong to the two particles taken separately, namely a greater or less *capacity for doing work* in virtue of the mutual action of the two particles upon each other. The *measure* of this capacity for doing work is termed the *potential energy of the system*, or the *relative potential energy of the two particles*; and that quantity of work serves as the *measure of this working-power* which is done in consequence of the mutual action of the two particles during their transference *from the smaller distance*  $r = \rho$  *to the greater distance*  $r = \infty$ , where  $\rho$  is determined by the particles themselves  $e, e'$ , by their masses  $\epsilon, \epsilon'$ , and by the constant  $c$ .

(5) The principle of the conservation of energy, however, when specially defined as above, is only applicable to two particles when their *potential* is of the same form as that of two electrical particles, namely

$$V = \frac{ee'}{r} \left( \frac{1}{cc} \cdot \frac{dr^2}{dt^2} - 1 \right).$$

The potential of two ponderable masses  $m, m'$ , on the contrary, is

$$V = \frac{mm'}{r},$$

which (neglecting the sign) can be included under the above general form only if the value of the constant  $c$  for ponderable masses is infinitely great. It is evident, however, that it would in reality suffice for the constant  $c$  to have only a very great value instead of an infinite value, in order that there might not be any thing perceptibly inconsistent with the results of experiment. And, considering the extraordinarily high value which must be ascribed to the constant  $c$  in the case of electrical particles, it does not seem at all necessary, for the avoidance of all sensible contradictions, to adopt any other value for ponderable bodies; consequently it must be permissible to represent the *potential* of two ponderable particles  $m, m'$  by

$$V = \frac{mm'}{r} \left( 1 - \frac{1}{cc} \cdot \frac{dr^2}{dt^2} \right),$$

where the constant  $c$  retains the same value as in the potential of two electrical particles.

But even if it should hereafter result from more accurate experimental results that it is not permissible thus to ascribe the same value to the constant  $c$  in the case of ponderable particles, the possibility would always remain of assigning to the constant  $c$  a still greater value for ponderable particles; and this could easily be taken so great that any sensible disagreement with experiment would completely vanish.

[To be continued.]

## II. Further Notes on the Theory of the Tides.

By the Rev. T. K. ABBOTT, Trinity College, Dublin\*.

IN the demonstrations given in two previous papers in this Magazine (January 1870 and February 1871), we have supposed the water to be limited to an equatorial canal, the moon also being in the equator. It is desirable to consider what modifications will be introduced, first, by supposing the earth to be uniformly covered with water, and, secondly, by taking into account the moon's declination.

It will save repetition if we state once for all certain general principles which we shall have to employ. First, suppose an accelerating force acts alternately in opposite directions, the effect (measured by velocity) increases as long as the force acts in either direction, and therefore the velocity in that direction is greatest at the moment that the force changes its direction. Secondly, the velocity (diminishing under the counteraction of the new force) continues to be in the same direction until this counter force has undone all the work done in that direction by the previous force. When the circumstances are alike in both directions, this will be when the force has done half its work. This is precisely the case of the common pendulum. Thirdly, in the case before us, the water rises when the particles behind are moving faster than those before. The rate of rise is greatest when this difference is greatest; but as the effect is cumulative, the whole amount of the rise is greatest at the moment when the difference = 0, and is about to change to the opposite. Fourthly, as in 2, this difference ceases to increase (*i. e.* is greatest) when the force (or difference of forces) producing it ceases to act; but it is not reduced to 0 until the opposite force has done half its work. At this moment the accumulation is greatest. Fifthly, in the case which we are now considering, the effective force depends on the form of the surface, and *vice*

\* Communicated by the Author.

*versé*. If, then, when this form is spherical the difference mentioned in 3 were always in the same direction, it would continue to act until a certain permanent alteration was produced. If the difference were constant in amount, a state of equilibrium would be attained; but if it alternately increases and diminishes, then the mean form of the surface will be the same that would be produced by a constant force equal to the mean amount of the actual force. The alternate excess and defect of the latter will cause a periodical motion, just as if it were an independent force\*.

First, then, the moon being still supposed to be in the equator, let the earth be uniformly covered with water. The tangential force may be resolved into two components, one touching the parallel of latitude (*i. e.* east and west), the other meridional†.

These may be regarded as giving rise to two distinct waves—one north and south, the other east and west. Now, with respect to the latter, the reasoning in the first paper (in the case of an equatorial canal with the moon in its plane) still holds good; and if this force were alone (that is, if the water moved in canals parallel to the equator), the ocean in every circle of latitude would take the form of an ellipse, with its short axis towards the moon. All the axes in this direction being shortened, and those at right angles being elongated similarly (for this component varies according to the same law in every latitude), the entire ocean would assume the form of an ellipsoid with its greatest and least axes in the plane of the equator, the least being directed towards the moon. The polar diameter being unaffected, would be the mean axis of the ellipsoid.

The effect of the meridional component is of a different kind. This constantly acts in the same direction, *viz.* towards the equator, and therefore causes an accumulation there corresponding to its mean amount, and a proportionate depression at the poles. From the equator to lat.  $45^\circ$  it is an elevating force, being greater as the particles are further from the equator; from that to the poles it is depressing. In every case, however, the force is in excess of the mean for half a rotation, *viz.* from  $45^\circ$  on each side of the meridian under the moon, and in defect in

\* If the reader wishes to apply these considerations to the case of an equatorial canal treated in the first paper, it must be observed that there the elevating force is the excess of easterly force acting on any particles of water above that which affects those in advance, *i. e.* to the east of them. This excess is positive from  $45^\circ$  west of the moon to  $45^\circ$  east (*i. e.* while the moon passes from  $45^\circ$  east zenith distance to  $45^\circ$  west), then negative for  $90^\circ$ , and so on.

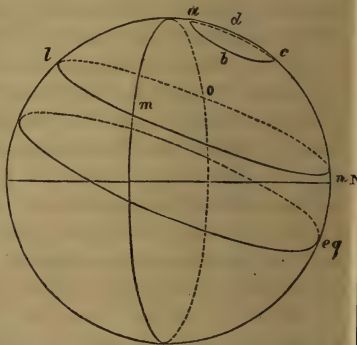
† The equatorial component is proportional to  $\cos \text{lat.} \sin 2 (\text{hour-angle of moon from meridian})$ ; the meridional to  $\sin 2 \text{lat.} \cos^2 (\text{hour-angle of moon from meridian})$ .



the remaining quadrants—being greatest when the moon is in the meridian, and zero when it is in the horizon. Hence, by 5 and 4, the elevation at the equator (and up to lat.  $45^\circ$ ) will be greatest (*i. e.* it will be high water)  $90^\circ$  from the moon. Beyond lat.  $45^\circ$  the depression will be greatest under the same circumstances. In these latitudes, therefore, the effect of the former component would be partially counteracted. It is easy, however, to see that the variation in the meridional force (which alone affects the tide) is in any latitude less than that in the force parallel to the equator (in the proportion of  $\sin \text{lat.}$  to 1); so that while the height of the tide would be lessened, the place of high water would be as before. It would be easy to calculate the height resulting from both these components combined. The form of the surface would be nearly but not exactly ellipsoidal, with the greatest axis equatorial and perpendicular to the line joining the centres of the earth and moon\*.

Let us now consider the case of the moon having a declination, which for simplicity I shall suppose less than  $22^\circ 30'$ . This limitation will not affect our results. We shall, as before, take the two components separately.

With respect, then, to the component which acts parallel to the equator. Near the equator itself the considerations applied in the former paper still hold good. Next consider a place,  $\alpha$ , whose polar distance is less than the moon's declination, to which therefore the moon is circumpolar, and (with the assumed declination) alternately north and south of the zenith. If  $abcd$  be the circle of rotation of such a place, it is obvious that the water will be accelerated through the whole semicircle  $abc$ , and retarded through  $cda$ . The same reasoning as already employed will show that it will be low water at  $c$  and high water at  $a$ . Now take an intermediate place whose circle of rotation is  $lmno$ . Here the water is retarded and rising from  $l$  to  $m$  and from  $n$  to  $o$ ; and accelerated and falling from  $m$  to  $n$  and from  $o$  to  $l$ , and the interval  $olm$  is less than  $mno$ . Hence the tide is lowest at  $n$  and not so low at  $l$ , and it is high water at  $m$  and  $o$ †.



The meridional component at the equator acts during half a

\* It is evident that, apart from the meridional force, the equatorial wave would itself be accompanied by a slight north and south oscillation.

† In the figure M is the point under the moon, N its antipodes.

rotation northward, and during the other half southward, and in each case is an elevating force, which, as before, has its greatest effect  $90^\circ$  from the moon. At all places whose latitude is less than the moon's declination there is a permanent accumulation. In the circle  $abcd$  this component is directed towards the north at  $a$  and towards the south at  $c$ , the points of change being where the great circles from  $M$  touch  $abcd$ . This gives rise to a north and south oscillation. The southerly force being the greater, there will be a residual depression of the water in this region. The depressing force, however, varies, being greatest at  $a$  and at  $c^*$ , while the elevating force is greatest where the tangents from  $M$  meet the circle. Hence, by 4 and 5, the tide will be lowest at the latter points and high at the former. Combining this with the former result, the effect of both components together will be to give high water at  $a$ .

It is not necessary to enter into a detailed examination of the state of things at intermediate places. It is not difficult to see that, as long as the moon's declination is small, there will be an accumulation effected by the meridional component extending from the equator to about lat.  $45^\circ$ , and that, as the moon's declination increases, the accumulation becomes less at the equator and greater towards  $45^\circ$ . If the declination were exactly  $45^\circ$ , there would be no accumulation at the equator, but two elevated rings at lat.  $45^\circ$ . With a greater declination these rings would approach the poles; and obviously, if the moon were at the pole, the ocean would take the form of a prolate spheroid.

The places of high water at any latitude, as far as this is due to the meridional component, would be easily found; but the proportionate effect of the meridional and equatorial components depends partly on the latitude and partly on the moon's declination; and it does not come within the scope of the present paper to solve this problem. It is sufficient to observe that the importance of the meridional component increases with the declination as well as with the latitude. If the moon were at the pole this force would be alone; and whatever the declination, it alone produces an effect at the pole.

\* If the moon's declination were greater than  $22^\circ 30'$ ,  $c$  might be less than  $45^\circ$  from  $M$ , in which case the force there would be an elevating one. Again, at a place whose latitude was greater than  $22^\circ 30'$  and less than the moon's declination, the moon's least nadir distance ( $=IN$ ) would be greater than  $45^\circ$ , and the force depressing.

III. *On the Mathematical Theory of Atmospheric Tides.* By the  
Rev. Professor CHALLIS, M.A., LL.D., F.R.S., F.R.A.S.\*

THE object of this communication is to indicate a method of deriving the solution of the Problem of Atmospheric Tides from the general equations of Hydrodynamics. I have already applied the same method to Oceanic Tides, on the particular suppositions that the whole of the earth's surface is covered by water of uniform depth, and that the body which by its attraction produces the tides revolves in the plane of the earth's equator. (See two articles in the Numbers of the Philosophical Magazine for January and April 1870, and a Supplement in the Number for June 1870.) The problem with these limitations is one which we must know how to treat mathematically before we can hope to arrive at a general theory of tidal motion. But although in point of generality this problem is a step in advance of that in which the hypothesis of an "equatorial canal" is made, its solution falls far short of giving results in accordance with the facts of nature, on account both of the irregularities of the ocean-bed, and of the interruption of the water-surface by islands and continents. The case, however, is not the same with respect to the atmosphere, which may be regarded as a fluid envelope of nearly uniform thickness, covering the whole of the earth, and of such height that its tides are but little affected by the irregularities of the earth's surface. Accordingly the following mathematical treatment of the tides of the atmosphere is closely analogous to that which was applied (I think, not unsuccessfully) to the above-mentioned hypothetical case of oceanic tides.

As it is not my intention to discuss the problem completely, but rather to demonstrate the applicability of a particular process of reasoning, I make, for the sake of simplicity, the following suppositions:—(1) The inferior boundary of the atmosphere is a spherical surface the radius of which is equal to the earth's mean radius; (2) the attracting body is the moon revolving eastward about the earth in the plane of the equator at its mean distance with its mean angular velocity; (3) the earth has no motion of revolution, the moon being conceived to revolve about it westward with the mean relative angular velocity ( $\mu$ ). As tidal motion is oscillatory,  $u dx + v dy + w dz$  is assumed to be an exact differential ( $d\phi$ ). Centrifugal force will be left out of account, as having, under the above conditions, no appreciable effect on the oscillations. The relation between the pressure ( $p$ ) and density ( $\rho$ ) is assumed to be always  $p = a^2 \rho$  at all points; so that the effect of variations of temperature is not considered. This

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being premised, the following known differential equations to the first order of small quantities, expressed in the usual notation, will be employed for the determination of the motion and pressure:—

$$\frac{d^2\phi}{a^2dt^2} = \frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} + \frac{d^2\phi}{dz^2}, \quad . \quad . \quad . \quad (1)$$

$$-\frac{a^2(d\rho)}{\rho} = Xdx + Ydy + Zdz - d \cdot \frac{d\phi}{dt}. \quad (2)$$

The earth's centre being the origin of rectangular coordinates, let  $\lambda$  be the north latitude, and  $\theta$  the longitude west from Greenwich, at the time  $t$ , of any particle of the fluid distant by  $r$  from the origin. Then

$$x = r \cos \lambda \cos \theta, \quad y = r \cos \lambda \sin \theta, \quad z = r \sin \lambda.$$

The impressed forces  $X, Y, Z$  are the resolved parts of the earth's attraction, and of the moon's attraction *relative* to her attraction on a particle at the earth's centre. Hence, if  $G$  and  $m$  be respectively the attractions of earth and moon at the unit of distance,  $R$  the moon's distance from the earth's centre, and  $\mu t$  her angular distance westward from the meridian of Greenwich,  $t$  being the time reckoned from the Greenwich transit, the following equations may be obtained by the usual process, powers of the ratio of  $r$  to  $R$  above the first being neglected:—

$$X = -\frac{Gx}{r^3} + \frac{m}{R^3} \left( x(3 \cos^2 \mu t - 1) + \frac{3y}{2} \sin \mu t \right),$$

$$Y = -\frac{Gy}{r^3} + \frac{m}{R^3} \left( y(3 \sin^2 \mu t - 1) + \frac{3x}{2} \sin \mu t \right),$$

$$Z = -\frac{Gz}{r^3} - \frac{mz}{R^3}.$$

Consequently  $Xdx + Ydy + Zdz$  is an exact differential, and the result of integrating the equation (2), regard being had to the expressions for  $x, y$ , and  $z$ , will be found to be

$$a^2 \text{Nap. log } \rho = \frac{G}{r} + \frac{mr^2}{2R^3} \left( 3 \cos^2 \lambda \cos^2 (\theta - \mu t) - 1 \right) - \frac{d\phi}{dt} + \psi(t). \quad (3)$$

It will now be convenient to employ the equation (1) under the form it takes when its coordinates are transformed into the polar coordinates  $r, \theta$ , and  $\lambda$ . This form of the equation is, as is known,

$$\frac{d^2 \cdot r\phi}{a^2 dt^2} = \frac{d^2 \cdot r\phi}{dr^2} + \frac{1}{r^2 \cos^2 \lambda} \frac{d^2 \cdot r\phi}{d\theta^2} + \frac{1}{r^2} \frac{d^2 \cdot r\phi}{d\lambda^2} - \frac{\tan \lambda}{r} \frac{d \cdot r\phi}{d\lambda}.$$

The next step is to obtain, by a particular solution of this equa-

tion, the expression for  $\phi$  which is appropriate to the present problem. This might, I think, be effected by means of Laplace's coefficients; but in the treatment of the problem of oceanic tides I was led to the required expression for  $\phi$  by a particular process, which is given at length in the articles in the *Philosophical Magazine* already cited. The solution in that instance suggested the form of expression which I now assume, namely

$$r\phi = f(r) \cos^2 \lambda \sin 2(\theta - \mu t).$$

This value of  $r\phi$  will be found to satisfy the foregoing equation, provided the form of  $f(r)$  be determined by integrating the equation

$$\frac{d^2 f}{dr^2} - \left(6 - \frac{4\mu^2}{a^2}\right) \frac{f}{r^2} = 0.$$

Putting for shortness' sake  $q$  for  $\left(1 - \frac{16\mu^2}{25a^2}\right)^{\frac{1}{2}}$ , the integration gives

$$f(r) = r^{\frac{1}{2}} (Cr^{\frac{5q}{2}} + C'r^{-\frac{5q}{2}}),$$

$C$  and  $C'$  being arbitrary constants. Consequently

$$\phi = r^{-\frac{1}{2}} (Cr^{\frac{5q}{2}} + C'r^{-\frac{5q}{2}}) \cos^2 \lambda \sin 2(\theta - \mu t).$$

I take occasion here to say that this value of  $\phi$ , which will subsequently appear to be indispensable for accounting theoretically for the phenomena of atmospheric tides, has not, as far as I am aware, been obtained before.

The fraction  $\frac{16\mu^2}{25a^2}$  is so exceedingly small that without sensible error  $q=1$ . Hence, for the present purpose,

$$\phi = (Cr^2 + C'r^{-3}) \cos^2 \lambda \sin 2(\theta - \mu t). \quad . \quad . \quad . \quad (4)$$

Now let  $u'$  be the velocity of the particle in the direction of  $r$  produced,  $v'$  its velocity in the direction *westward* from the meridian passing through its position, and  $w'$  the velocity *northward* along that meridian, so that

$$(d\phi) = u'dr + r \cos \lambda v'd\theta + rw'd\lambda,$$

an exact differential, because  $u'dx + v'dy + w'dz$  is an exact differential, whatever be the directions of the axes of rectangular coordinates. Hence, from the equation (4),

$$u' = \frac{d\phi}{dr} = (2Cr - 3C'r^{-4}) \cos^2 \lambda \sin 2(\theta - \mu t),$$

$$v' = \frac{1}{r \cos \lambda} \frac{d\phi}{d\theta} = 2(Cr + C'r^{-4}) \cos \lambda \cos 2(\theta - \mu t),$$

$$w' = \frac{1}{r} \frac{d\phi}{d\lambda} = -(Cr + C'r^{-4}) \sin 2\lambda \sin 2(\theta - \mu t).$$

Also

$$\frac{d\phi}{dt} = -2\mu(Cr^2 + C'r^{-3}) \cos^2 \lambda \cos 2(\theta - \mu t).$$

To obtain the motion and density of the fluid at any point, it is now only required to find the values of the arbitrary quantities  $C$ ,  $C'$ , and  $\psi(t)$  from the given conditions of the problem.

One condition is that at the earth's surface  $u'$  is constantly zero. Hence, if the earth's radius  $= b$ , we have

$$2Cb - 3C'b^{-4} = 0, \text{ or } \frac{C'}{C} = \frac{2b^5}{3}.$$

Hence, by eliminating  $C'$ ,

$$\frac{d\phi}{dr} = 2C \left( r - \frac{b^5}{r^4} \right) \cos^2 \lambda \sin 2(\theta - \mu t),$$

and

$$\frac{d\phi}{dt} = -2\mu C \left( r^2 + \frac{2b^5}{3r^3} \right) \cos^2 \lambda \cos 2(\theta - \mu t).$$

For determining  $\psi(t)$  we may employ a condition indicated by the foregoing expressions for  $u'$ ,  $v'$ ,  $w'$ , and  $\frac{d\phi}{dt}$ , namely that

these quantities are all constantly zero where  $\lambda = \frac{\pi}{2}$ —that is, at the pole of the earth and in the fluid column incumbent upon it. Consequently the density of the fluid at the pole will be constant; and if  $\Delta$  be its value, we have, by the equation (3),

$$a^2 \text{Nap. log } \Delta = \frac{G}{b} - \frac{mb^2}{2R^3} + \psi(t),$$

which proves that  $\psi(t)$  is independent of the time. By eliminating this quantity, and substituting the foregoing value of  $\frac{d\phi}{dt}$ , the equation (3) becomes

$$a^2 \text{Nap. log } \frac{\rho}{\Delta} = -G \left( \frac{1}{b} - \frac{1}{r} \right) + \frac{m}{2R^3} \left( b^2 + \frac{r^2}{2} (1 - 3 \sin^2 \lambda) \right) + \left( \frac{3mr^2}{4R^3} + 2\mu C \left( r^2 + \frac{2b^5}{3r^3} \right) \right) \cos^2 \lambda \cos 2(\theta - \mu t). \quad (5)$$

The third condition necessarily has reference to the circumstances of the atmosphere at its superior limit. On the hypothesis of the atomic constitution of bodies, it may be shown as follows that at a certain height the atmosphere must terminate abruptly. Conceive a horizontal surface to be drawn through the position of a given atom. Then, on that hypothesis, the upward accelerative force due to the molecular action of the



atoms below the surface will exceed the downward accelerative force due to that of the atoms above by just the accelerative force of gravity. Consequently, if by reason of the diminution of the density the former force eventually becomes only equal to the force of gravity, it is evident that there can be no more downward molecular action, and that thus a superior limit of the fluid will be reached. Also, since at the very boundary the density cannot be finite, there will be an abnormal increase of density downwards from the boundary, where it is zero, to a certain small distance at which the variation of density becomes regular—that is, unaffected by the abrupt termination. Within this stratum, which, although extremely thin, must be supposed to exceed in extent the sphere of molecular action on a given atom, the variation of density satisfies the condition of making the upward molecular action on *each* atom exceed the downward by just the force of gravity. If  $\delta$  be the density at the distance  $b'$ , where the variation ceases to be abnormal, the result there of the combined action of the molecular forces and the force of gravity is equivalent to a pressure  $a^2\delta$  applied at all points of the surface of radius  $b'$ , and the terminal density may, without sensible error, be supposed to have the finite value  $\delta$ . Although this reasoning applies strictly to a state of equilibrium of the atmosphere, it will clearly not be perceptibly affected by the slow oscillatory motions we are considering.

It may here be stated that the idea of a particular molecular condition at the superior boundary of the atmosphere was entertained by Poisson, and that it was regarded by him as analogous to a gradation of superficial density assumed to exist at the surfaces of liquids and solids. In these substances, however, the gradation of density would be maintained by combined *molecular* attraction and repulsion, independently of the force of gravity.

Supposing, therefore,  $b'$  to be the value of  $r$  for the top of the aerial column which has its base at the earth's pole, according to the foregoing argument the equation (5) gives

$$a^2 \text{ Nap. log } \frac{\delta}{\Delta} = -G \left( \frac{1}{b} - \frac{1}{b'} \right) - \frac{m}{2R^3} (b'^2 - b^2),$$

which determines the relation between the terminal density  $\delta$  and  $b' - b$  the height of the atmosphere.

This being understood, since a particle at the superior surface may be assumed to remain at the surface in successive instants, we shall have, with respect to such a particle,  $\left( \frac{d\rho}{dt} \right) = 0$ , because the superficial density will at all points be the same in successive instants. Hence, differentiating the equation (5) with respect

both to space and time, putting for  $\frac{dr}{dt}$  or  $\frac{d\phi}{dr}$  the foregoing value, and  $b'$  for  $r$ , and omitting terms of the second order with respect to  $m$ , it will be found that the condition  $\left(\frac{dp}{dt}\right) = 0$  gives the following equation for determining  $C$  :—

$$C \left( 1 - \frac{b^5}{b'^5} - \frac{2\mu^2 b'^3}{G} \left( 1 + \frac{2b^5}{3b'^5} \right) \right) = \frac{3m\mu b'^3}{4GR^3}.$$

Thus the three arbitrary constants have been determined, and at the same time the general hydrodynamical equations (1) and (2) are satisfied. It might hence be argued that the solution we have obtained is by this means proved to be the true one, inasmuch as only one solution can satisfy *all* the conditions required to be fulfilled. It will, however, be worth while to test this inference by other considerations.

First, it may be remarked that  $C$  becomes infinite if the factor which multiplies it vanishes. Putting  $x$  for  $\frac{b'}{b}$ , this will be the case if

$$x^5 - 1 = \frac{2\mu^2 b}{g} \left( x^3 + \frac{2x^3}{3} \right).$$

Since  $g = 32.191$  feet,  $\mu = \frac{79}{82} \times$  the earth's rotation in one second, and  $b = 3956$  miles, it follows that  $\frac{2\mu^2 b}{g} = \frac{1}{155.7}$ , and the value of  $x$  which satisfies the above equation will be found to be  $1 + \frac{1}{461}$  nearly. Consequently

$$b' - b = \frac{3956}{461} = 8.58 \text{ miles nearly.}$$

Hence, if the height of the atmosphere had this particular value,  $C$  would be infinite, and there would be unlimited tide. According as  $C$  is positive or negative,  $b' - b$  is greater or less than this quantity. Hence for the atmosphere  $C$  is positive, its height being known to be much greater than 8.6 miles.

It is remarkable that the theory of oceanic tides conducted to a like critical value of the depth of the ocean, and in amount very nearly the same as this critical height of the atmosphere. The reason is, that for this particular depth of the ocean, or height of the atmosphere, the rate of propagation of waves, as due to the action of gravity independently of the elasticity of the medium, is very nearly the same as the rate of the relative rota-

tion of the moon about the earth, in consequence of which the moon's attraction might produce an accumulation of waves to an unlimited extent. That the rate of this kind of propagation is independent of the density of the medium is proved experimentally by the fact that mercury and water are propagated at the same rate in a rectangular trough by the action of gravity, if only the depths of the fluids be the same. With respect to an unlimited ocean, if the uniform depth be less than the critical value 8.5 miles, as is the case for the mean depth of the actual ocean,  $C$  is negative.

If after putting  $\delta$  for  $\rho$  in the equation (5) the value obtained above be substituted for  $a^2 \text{Nap. log } \frac{\delta}{\Delta}$ , and if  $b'$  be put for  $r$  in the small terms, the equation of the upper surface of the atmosphere will be found to be

$$r = b' + \frac{3mb'^4}{4GR^3} \cos^2 \lambda + \frac{Cb'}{\mu} \left(1 - \frac{b^5}{b'^5}\right) \cos^2 \lambda \cos 2(\theta - \mu t).$$

Hence as  $C$  is positive, high tide occurs when  $\theta - \mu t = 0$ —that is, at syzygies; and low tide when  $\theta - \mu t = \frac{\pi}{2}$ , or at quadratures.

(The contrary is the case for ocean tides, because with respect to the actual ocean  $C$  is negative.) The difference between the high and low tides at the equator is

$$\frac{2Cb'}{\mu} \left(1 - \frac{b^5}{b'^5}\right).$$

Again, supposing  $\rho'$  to be the density of the atmosphere at any point of the earth's surface, the same equation (5) gives

$$a^2 \text{Nap. log } \frac{\rho'}{\Delta} = \frac{3mb^2}{4R^3} \cos^2 \lambda + \left(\frac{3mb^2}{4R^3} + \frac{10Cb^2}{3}\right) \cos^2 \lambda \cos 2(\theta - \mu t).$$

On the equator let  $\rho' = \Delta(1 + \epsilon_1)$  at syzygies, and  $\rho' = \Delta(1 + \epsilon_2)$  at quadratures,  $\epsilon_1$  and  $\epsilon_2$  being extremely small fractions. Then, if we suppose that  $a^2 = ghD$ ,  $h$  being the mean height of the mercury column and  $D$  its density, we shall have for calculating  $h(\epsilon_1 - \epsilon_2)$ , which is the excess of the height of the barometer at syzygies above that at quadratures, the formula

$$\frac{\Delta b}{Dg} \left(\frac{3mb}{2R^3} + \frac{20\mu Cb}{3}\right).$$

To obtain the foregoing results in an arithmetical form, it would be necessary to ascertain the numerical value of  $C$ . As this constant depends on the height of the earth's atmosphere, which is an unknown element, I propose to perform the calculations on the hypothesis that the height of the atmosphere is sixty miles.



For this purpose we have  $b=3956$  miles,  $b'=4016$  miles,  
 $\mu = \frac{79}{82} \times$  the earth's rate of rotation, and  $\frac{\mu^2 b^3}{G} = \frac{\mu^2 b}{g} = \frac{1}{311.4}$ ;  
 and it will be assumed that  $\frac{b}{R} = \frac{1}{60.3}$  and  $\frac{m}{G} = \frac{1}{70}$ . With  
 these data the calculation from the formula for  $C$  gives  
 $C = 0.0000008296$ . Hence from the foregoing formulæ the  
 following results may be obtained:—

For the equation of the surface,

$$r - b' = 1.0841 \text{ ft. } \cos^2 \lambda + 1.2753 \text{ ft. } \cos^2 \lambda \cos 2(\theta - \mu t);$$

The difference between high and low tide = 2.5507 ft.;

The barometer is higher at syzygies than at quadratures by  
 0.00278 in.

Also, from the equation which gave the relation between the  
 terminal density  $\delta$  and the height  $b' - b$  of the atmosphere, it is  
 found, by calculating on the supposition of a height of sixty miles,  
 that  $\delta$  is equal to one six-millionth part of the density  $\Delta$  at the  
 surface.

These results do not admit of comparison with observation,  
 excepting in the case of the difference of barometric heights.  
 By observations made at St. Helena, first by Captain Lefroy and  
 afterwards by Captain Smythe, the details of which are given by  
 Sir Edward Sabine in the *Philosophical Transactions* for 1847  
 (p. 45), it appears that the height was greater at syzygy than  
 at quadrature by 0.00365 in. Nearly contemporaneous obser-  
 vations (1841–45) made by Captain Elliott at Singapore gave a  
 difference equal to 0.00570 in. (see the *Philosophical Transac-*  
*tions* for 1852, p. 125). Both these values exceed that given  
 by the theory on the hypothesis of an atmosphere sixty miles  
 high. By assuming a less height a nearer agreement with ob-  
 servation would be obtained. So far, therefore, as the theory  
 is trustworthy, we may infer from it that the height of the  
 atmosphere is less than sixty miles.

Thus, although the theory cannot be put to a strict numerical  
 test, inasmuch as it makes the high tide of the atmosphere  
 occur under the moon it is so far confirmed by observation. I  
 think also that it may be regarded as no little confirmation of  
 the theory that it explains why in this particular the tide of the  
 atmosphere differs from that of an unlimited ocean of uniform  
 depth less than 8.5 miles, for which there would be low tide  
 under the moon.

The foregoing paper was read in the Mathematical Section, at  
 the Meeting of the British Association at Edinburgh; and an

abstract of it will appear in the 'Report' for 1871. My reason for publishing it *in extenso* at the present time is that I propose to make use of the results relating to the variations of atmospheric pressure due to the moon's attraction in a discussion of the hydrodynamical theory of *magnetism*, and was desirous of previously exhibiting the mathematical reasoning by which the results are obtained. In my work on the Principles of Mathematics and Physics (pp. 662-665), I have attempted to account for the lunar diurnal variation of terrestrial magnetism by attributing it to the gradations of the pressure of the earth's atmosphere caused by the moon's gravitation; but at that time I was not acquainted with the proper method of ascertaining theoretically the laws of the moon's disturbance of the atmosphere. This problem having been subsequently solved by the method explained in this communication, I then found that the resulting laws and amount of the variation of the atmospheric pressure fail to account for the observed laws of lunar diurnal variation of magnetism. The consequent necessity of abandoning this mode of explaining the phenomena has led to a considerable modification of the views contained in the same work (pp. 670-676) respecting the causes of *cosmical magnetism*, the particular reasons for which I hope to be able shortly to explain in the course of a general review of the Hydrodynamical Theory of Magnetism.

Cambridge, December 8, 1871.

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IV. *Description of a new Anemometer for Indicating and Registering the Force and Direction of the Wind at any distance from the Vane, &c., the communication being made by means of Electric Wires and without the aid of Shafting. Invented by J. E. H. GORDON, late of King's College, London\*.*

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[With a Plate.]

THE object of this instrument is to do away with the necessity of the shaft hitherto required for anemometers indicating at a distance. Several anemometers for communicating force by electricity have been invented in the last year or two; but this is, I think, the first time that direction also has been communicated and printed by the electric current†.

\* Communicated by the Author.

† Since the above was in type the Rev. S. J. Perry has had the kindness to send me a description of Padre Secchi's *Météorographe*, which contains an electrical anemometer. In this (to quote Padre Secchi's description),

.... "La direction du vent est enregistrée par quatre télégraphes. Elle est obtenue au moyen d'une girouette à la proue de laquelle on donne une forme angulaire afin de diminuer les oscillations. Au pied de la

The best direction-instrument at present in use is that invented by Messrs. Beckley and Casella, in which a moving chain is substituted for the shaft. In this, however, there must, I think, be some momentum, and the chains must pass in an approximately straight line from the vane to the printing-instrument.

In the electrical anemometer there is no momentum whatever. The (four) wires being stationary, can pass anywhere where most convenient; for instance they can come down outside the house and enter through a gimlet hole in the window-frame.

The cups and fans can be placed at any distance from the printing-instrument. For instance, the vane might be at Portsmouth and the printing-instrument in the meteorological office in Westminster\*. Very small battery-power is required; six Walker cells are used, with acid diluted 14 to 1, equal to rather less than one Grove's cell.

The direction-apparatus is moved by a set of Beckley fans, the force by a set of Robinson's cups.

The mechanism is as follows:—

#### *The Direction Contact-apparatus.*

On the axis (Plate I. fig. 1) of the fans is a crown cog-wheel with 256 teeth, which gear into a wheel of 16 teeth, giving a multiplying-power of  $\frac{256}{16} = 16$ , thus causing a horizontal axis to revolve once every time the hand moves a point—that is, counting

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gironette, et placée à l'air libre, est un rose de quatre secteurs métalliques garnis de platine, contre laquelle vient s'appuyer une languette fixée sur l'arbre de la girouette.

“L'appareil est muni de quatre télégraphes dont les électros sont respectivement en communication avec les quatre secteurs; chacun des quatre télégraphes, en faisant osciller son levier selon la direction dans laquelle la girouette ferme le circuit, donne l'un des quatre vents principaux. Les vents intermédiaires aux quatre principaux s'obtiennent par la combinaison des deux voisins. Cette combinaison se produit soit par l'oscillation de la girouette, soit par l'indication simultanée de deux télégraphes.

“L'expérience a prouvé que dans la pratique ce système satisfait aux besoins de la science météorologique actuelle.”—*Etudes Religieuses, Historiques et Littéraires*, par les Pères de la Compagnie de Jésus.

In this it will be seen that five wires (four line and, of course, a return) are employed to give 8 points of the compass. My instrument gives 16 or 32 points with three wires. The paper on which the wind is recorded in Padre Secchi's instrument has to be changed every ten days. The instrument also, as far as I understand, works with a closed circuit. The contact-breaker, however, is no doubt simpler than mine, and has the advantage of using a vane instead of Beckley fans.—J. E. H. G., December 16, 1871.

\* In the instrument lately erected at Eton the fans and cups are 45 yards from the registering instrument.



16 points to the compass; the instrument can, however, be made to give 32 or 64 if required. On this axis are two contact-breakers; one makes a contact for each complete revolution in one direction, the other for each complete revolution in the other direction.

The contact-breakers consist of two ebonite disks. Fig. 2 is a front view of one of the disks. In the surface of it are turned two grooves—one concentric with the axis of the disk, the other excentric and of larger radius than the first; the two grooves run into each other for rather more than half their circumference. In the grooves are two slides or valves; these move on a pin at one end, the other end has a piece of wire fastened to it; this wire passes through a little curved slot at the bottom of the groove (the radius of curvature equals the length of the valve); the wire projects right through the slot, and is pressed on by a spring fixed on the back of the disk. In one of the disks the valves are pressed by the springs into the positions shown in fig. 2; in the other disk the springs press them in the opposite direction.

Near the disks are slips of thin brass fixed on little upright pillars and insulated from one another. The ends of the brass slips are fixed at right angles to them, and dip into the grooves. The ends are made circular, and can revolve so as to lessen the friction against the sides of the grooves. When the disk  $\alpha$  revolves in the direction of the arrow, the point of its brass slip is caused by the valve to travel in the concentric inner groove and no effect is produced. When, however, the disk turns in the direction opposed to the arrow, the point travels in the excentric outer groove, the brass slip is depressed, a platinum button on it presses on a corresponding platinum point on a spring placed just below the brass slip, and a current passes between the points. The valves of the two disks are so arranged that the direction of motion which makes contact by one disk does not make contact by the other, and *vice versa*.

The weight at the left-hand side of fig. 1 is most important. It revolves with the disks, and prevents the vane ever stopping with either point in contact. If it were not for this, the points might remain in contact, and a battery which ought to last six months would be worked out in an hour or two. It also prevents the vane making a contact and then going back without passing a whole point.

In order that the weight may work, the crown wheel is not rigidly keyed on to the axis, but has two stops fixed on it a short distance apart; a pin fixed to the axis projects between the two stops. The crown wheel has a play which is made equal to half a revolution of the disks.

When the wind changes, the pin fixed on the vertical axis moves forward till it comes to one of the stops; then, pushing on the stop, it moves the crown wheel and causes the disks to revolve and raise the weight. As soon, however, as the weight has passed the top, it falls over, moving the crown wheel and causing the stop to gain a little on the pin; the disks then remain at rest till the pin has again overtaken the stop.

The disks are so adjusted that contact is always commenced after the weight has begun to fall, and finished before the weight has quite done falling.

Thus a perfectly uniform and steady contact is obtained.

*The Dial and Registering Instrument (figs. 3 and 4).*

*The Dial.*—On the stand of the dial instrument are two electromagnets placed horizontally. Opposite the poles of these are armatures hinged to the base; these form parts of levers, the upper parts of which are of thin brass.

At the top of each lever is a kind of brass claw hinged so that it can rise a little, but prevented by the hinge from falling below a certain position. Fixed rather lower down on the lever is another brass claw of the form shown; opposite to the claw is a ratchet-wheel with sixteen teeth (as shown). When a current passes in the magnet the armature is attracted, the point moves the ratchet-wheel on one tooth, the hinge allowing it to rise over the top of the wheel; at the same time the stop (or lower claw) prevents it moving more than one tooth. When the current ceases, the armature is drawn back by a spring; a set screw prevents the armature going back too far. The two wheels and the hand are fixed on one axis, the wheels being placed as near together as the thickness of the magnets will allow.

A slight spring with a friction-roller at the end of it is so arranged that the roller rests between two of the teeth of one of the ratchet-wheels; this keeps the wheel always in the right position for receiving the push on the lever. The wheels are made adjustable on the axis by means of set screws, partly that the roller in putting one wheel into position may also adjust the other, and partly that the hand may be made to rest at the even points.

One magnet drives the hand in one direction; the other drives it in the other direction.

This is the indicating part of the instrument; it can be used without the registering part. When registration is required the following arrangement is used.

*The Printing Apparatus.*—The axis of the hand is prolonged backwards, and a type-wheel embossed with the sixteen points of the compass is fixed on it in such a position that the point to

which the hand is pointing shall be at the bottom of the type-wheel. Close behind this, and with its lower edge coming a little below the lower edge of the direction type-wheel, is a wheel embossed with the hours from I to XII. This is fixed on the hour-axis of a rather powerful clock; a wheel of unpolished boxwood is kept by a spring pressed up against its lower edge\*. Underneath the direction-wheel, but not touching it, is a little stamper faced with boxwood; under the stamper is a lever, to which is attached an armature which rests over an electromagnet. Inside the clock, on one of the axes (which revolves once in an hour) is an ivory wheel with two small platinum studs on its edge opposite to each other; a little spring with a platinum point presses on its edge. This is connected to the stamper-magnet, and at each half hour sends through it a current of about two seconds' duration, which presses the stamper up against the direction type-wheel. The paper is wound on two large reels; it consists of a continuous band of white paper, and a similar band of black copying-paper. The two papers were at first wound together on one reel; but it was found, when they had been together for some time, that the oil from the black paper soaked into and dirtied the white paper. To avoid this Mr. Apps has arranged to wind the papers on separate reels, the reel of black paper being placed immediately above the reel of white paper. The papers pass between the hour type-wheel and its spring roller. The clock as it turns draws the papers through (the black being uppermost), and prints off the hours on the white paper. Each half hour the stamper is raised and prints off whatever letter on the direction-wheel is at the bottom at the moment.

The stamper, however, continues raised for nearly two seconds. If the wind were to change during that time the lever would not be able to move the hand, as it would be jammed by the stamper. To avoid this source of error, the current which works the driving-magnets is made, on its way, to pass round another little magnet; the armature of this magnet is kept by a spring against a platinum point. The current which works the stamper-magnet has on its way to pass from this point to the armature. Suppose the stamper to be raised and a current to pass round one of the driving-magnets; it passes round the safety-magnet, pulls the armature away from the platinum point, breaks the current in the stamper-magnet, releases the stamper, and allows the hand to move (see fig. 6).

#### *The Force-apparatus.*

The contact-breaker is shown in plan, fig. 5. It consists of a set of Robinson's cups, connected by the usual gearing to a box-

\* See note at end of paper.



wood wheel which revolves once for every mile of wind. This wheel has a deep rectangular groove cut round its circumference ; in it at one part is placed an inclined plane or wedge of gentle slope and square at the end ; as the wheel revolves, the pointed end of the wedge goes first. The height of the plane is a little less than the breadth of the groove. Just beyond the wedge, at the square end of it, a little piece of platinum is let into the bottom of the groove and connected through the axis of the disk to a binding-screw on the base.

Fixed on an upright is a lever (shown broken in the drawing) carrying a platinum pin, which dips into the groove and can play from side to side of it. An armature is attached to the lever, and a small electromagnet is placed near it.

In the registering instrument is an electromagnet, whose armature can slide up and down in vertical guides. A bar passes downwards from the armature, and terminates in a point just above the paper and between the two type-wheels. It is kept off the paper by a spring.

When the contact-disk revolves, the platinum dot travels forward till it reaches the pin attached to the lever ; a current then passes through both electromagnets. In the registering instrument a dot is made on the paper, while the contact-breaker magnet draws the lever across the groove and pulls it out of contact. When the disk has nearly completed another revolution the pin comes to the inclined plane, and is by it pushed back across the groove ready for another contact. By this method no work is expended in raising a weight or spring to prevent the points remaining in contact ; each dot on the paper thus represents one mile of wind. Thus the time, force, and direction of the wind are printed side by side. A specimen of the record is shown in fig. 4.

Instead of the two papers coming out together (in which case the record is hidden by the black paper for two days after being printed), the black paper immediately after leaving the type-wheels is carried upwards back over the top of the instrument and down behind the reels, where it passes out through a slot in the base. The papers are kept tight by little weights attached to their ends by clips.

I cannot conclude this paper without expressing my obligations to Mr. Apps for the skill and care which he has displayed in the manufacture of the instrument.

July 1871.

*Note added November 12, 1871.*

Four of these instruments have now been made, two printing and two simply indicating. They all act exceedingly well. Of

the printing instruments, one was erected in July at the new laboratory at Eton College; the other, made for Lord Lindsay, has not yet been erected, but has been at work for six or eight weeks at Mr. Apps's. In the last-named instrument a clock-face has been placed in front of the force printing-magnet, showing the hour being printed; and a lever-handle with a catch has been added for pressing down the spring roller beneath the hour-wheel for the purpose of introducing new papers easily. The reels hold a supply of paper for three or four months.

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V. *On the Mechanical Impossibility of the Descent of Glaciers by their Weight only.* By HENRY MOSELEY, Canon of Bristol, F.R.S., and Corresponding Member of the Institute of France, in answer to Mr. Mathews\*.

IN order that the question of the possibility of the descent of glaciers by their weight only may be discussed as one of exact science, it is necessary that some geometrical form be assigned to a glacier. I have assumed the simplest. I have imagined a glacier having a uniform rectangular cross section of equal roughness, and of a constant slope and direction. My argument is, 1st, That it is impossible such a glacier should descend with that differential motion which is characteristic of the descent of glaciers by its *weight only*; 2ndly, That, this being impossible with a glacier of a uniform section and constant slope and direction, it is impossible *à fortiori* with an actual glacier of a variable slope, section, and direction.

No question appears to me to have been raised by Mr. Mathews and the other gentlemen who have done me the honour to answer my paper, as to whether, admitting my first conclusion, the second necessarily follows from it. They have not denied that *if* it be true that a glacier could not descend by its weight in my imaginary channel, it follows *à fortiori* that it could not descend in a channel such as the actual channels of glaciers are. What I propose now to do is therefore to answer their objections (and specially those of Mr. Mathews) to my first conclusion, that in a uniform rectangular channel of constant direction and of a constant slope equal to that of the Mer de Glace it is impossible that a glacier should descend by its weight only. Mr. Mathews will, I think, admit that in the descent of such a glacier the motions of different points in a straight line drawn across its surface at right angles to its axis would be different but parallel to one another, like those of the surface of a stream of water flowing in such a channel—and that they would be different and parallel to

\* Communicated by the Author.

one another, not only upon the surface but beneath it, those on the surface that are nearer to the axis moving faster than those more remote, and those below the surface that are nearer to it moving faster than those deeper down\*.

This differential motion supposes the displacement of infinitely thin longitudinal strips of the ice in the direction of the length of the glacier side by side and over and under one another. The force (in the nature of pressure) which opposes itself to this displacement I call *shear*; and I obtain the *unit* of shear by a direct experiment of the resistance to the shearing of two parts of the same piece of ice upon one another by means of an apparatus specially constructed for that purpose†, precisely as in this glacier contiguous strips of ice are in their differential motion supposed to be sheared. I have not found this unit of shear to be in any case more than 3 lbs. less than 75 lbs. per square inch in ice presumably of the temperature of 32° Fahr.; but I have found it to be considerably greater in ice of a lower temperature‡.

The unit of shear being known, and the differential motion of the glacier, it is possible to determine the *work* expended on shearing through any distance of its descent. If its weight be the only force which causes it to descend, then the work of its weight through this descent must at least equal the work of its shearing. But it is actually far less than it; it is less than one thirtieth of it. It is impossible, therefore, that a glacier of uniform section and slope and direction should descend by its weight only; and being impossible with such a glacier, it is *à fortiori* impossible with a glacier of variable section and direction. This is my

\* If proof were needed of this fact, the ribboned structure of the interior supposes it to be maintained throughout such a glacier, and the dirt-bands of the surface would be sufficient evidence. See Phil. Mag. April 1870.

† See Phil. Mag. January 1870.

‡ See my paper in the Philosophical Magazine for August 1871. In describing an experiment made on February 15, 1870, I have spoken of a cylinder of ice when placed in the shearing-apparatus at 6¼ o'clock in the evening as having yielded, under a pressure of 63·6 lbs. per square inch on its *upper* surface, at 8 o'clock by  $\frac{1}{16}$  inch, and of its having been examined at 10½ the next morning and found not to have sheared during the night. I have failed to express myself clearly, and Mr. Mathews has misunderstood me. I should have explained that the yielding of the upper surface was accompanied by *no shearing* of one portion of the ice over the other. Before shearing begins in my apparatus, the pressure on the upper surface of the ice being greater than elsewhere, and the apparatus being warmer than the ice, the ice is melted there more than elsewhere. It was by this melting, and not by shearing, that the upper surface yielded. In experiments under an external temperature above freezing the warmth of the shearing-apparatus and its conductivity cause a continual melting of the ice and diminution of its cross section. When the ice once begins to shear, as the shearing-pressure remains constant while the section continually diminishes, the shearing motion cannot but be *kept up*.



argument. To follow it out completely, a somewhat difficult application of the principles of dynamics is required. The question of the descent of glaciers is indeed essentially one of mechanical philosophy, and, as such, cannot be argued with precision otherwise than by mathematical reasoning. It is a question which has nevertheless long been in possession of that kind of scientific opinion (as distinguished from exact science) which refuses to take account of mathematical reasoning. Unless I discard all but the most elementary forms of this kind of reasoning, I cannot therefore expect to be listened to by those who chiefly take an interest in the discussion. I will therefore attempt, although I confess with no very sanguine hope of success, to conduct my argument subject to this condition. Mr. Mathews will understand at what a disadvantage I thus place myself.

I will ask him to imagine a rectangular channel of ice to be cut out of my imaginary glacier of the same form and nearly of the same dimensions as the glacier itself, so that only a comparatively small thickness of ice, forming the sides and bottom of this ice-channel, shall lie between it and the rock. Before the ice was taken out of this ice-channel it descended with a differential motion. To fix ideas, let the channel be dug a mile long and open at both ends, and let the ice be imagined to be then replaced and the glacier reconstructed as follows. Let a strip of ice a foot square in section and a mile long be conceived to be placed at the bottom corner of the channel at its left-hand side looking down the glacier; and to be frozen by its side against the side of the channel, but not by its base against the bottom; so that, to be made to slide *down*, this ice-strip must be made to *shear* by its side only. Now the weight of each foot of the length of this ice-strip is  $62\frac{1}{2}$  lbs. nearly, being the weight of a cubic foot of water; and the glacier being inclined at an angle of  $4^{\circ} 52'$  to the horizon (being the inclination of the Mer de Glace), this  $62\frac{1}{2}$  lbs. pressure of each foot in the length of the strip *vertically* produces a pressure *down* the glacier of 62.5 lbs.  $\times \sin 4^{\circ} 52'$ , or of  $62.5 \text{ lbs.} \times .084837 = 5.3 \text{ lbs.}$  nearly. But the shearing-force per square inch being 75 lbs., each foot in the length of the strip being frozen to the side of the channel requires  $144 \times 75 \text{ lbs.}$ , or 10,800 lbs. to shear it. The strip, therefore, will not descend. Let now another strip of the same size be placed on the bottom of the glacier beside the first and frozen to its side, but not to the bottom, on which it is to be supposed free to slide without friction. The two strips thus frozen together will produce a shearing-force on the side of the channel of 5.3 lbs.  $\times 2$  per square foot, whereas the resistance to shearing is 10,800 lbs. per square foot. The two strips will still therefore remain *fixed* to it; and if strip after strip be added side by side in the

same way, the shearing-pressure of 10,800 lbs. per square foot will not be reached until 2037 strips have been so placed, and a platform of ice has been formed of that breadth in feet, resting without friction or adhesion on the bottom of the channel, but adhering by its edge to the side. An additional strip (the 2038th) will cause this platform to shear along its edge and to descend bodily. Let now a *resistance* be supposed to be applied to the first strip nearest to the side just sufficient to prevent it, and therefore the platform, from descending by the *run*, i. e. with an accelerated motion. Another strip (the 2039th) being then added at the outside edge of the platform, the second strip will be made to shear on the first; and the *acceleration* of its descent being resisted as before, the addition of another strip (the 2040th) will cause the third strip to shear on the second; and by continuing this process of accretion at the outside edge of the platform and of resistance\* at the inner edge, a differential motion of the strips at the inner edge will be set up (numbering from the first), and there will result from it a bodily descent of 2037 feet of unbroken platform beyond—the whole descending mass of ice being composed of a part near the side of the channel which descends with a differential motion, and of a part beyond it always 2037 feet wide which descends not with a differential motion but *bodily*. Every addition made to the outside edge of this last-mentioned part takes from as much of its inner edge its unbroken character and adds it to the part which descends differentially. Thus one half of the platform of a glacier is constructed. If the other half be similarly constructed, beginning from the opposite side of the channel, a complete platform will be obtained of which the central portion (4074 feet wide) descends bodily, i. e. without any differential motion, and of which two side portions, dependent for their width on the width of the channel, descend with a differential motion.

Let now the whole of this platform be supposed to be frozen to the bottom. Its descent will thus be arrested; but it will be ready to begin to descend again, as it did before, if by the operation of any other force the resistance to its shearing over the bottom of the channel shall be overcome. The thrust downwards with which the weight of each cubic foot of it tends to overcome this resistance to its shearing over the bottom, has been shown to be 5·3 lbs. per square foot, whereas the resistance itself

\* Such resistance, increasing in amount with the velocity of the descent, must be actually in operation in glaciers descending by their weight only, or they would descend *by the run*. It will be borne in mind that when systems of bodies acted on by given forces move under the action of those forces with uniform velocities, the forces have the same relation to one another as they would have if the bodies were at rest.

is 10,800 lbs. per square foot. It would therefore require (reasoning as before) 2037 such platforms piled horizontally upon one another, or a glacier at least 2037 feet in depth, to make the bottom of the glacier shear over the bottom of the channel. Every similar platform then added to the top would set free the ice an additional foot from the bottom, and set up from the bottom a differential motion of the ice horizontally, whilst at the same time the vertical differential motion, which was stopped when the glacier was frozen to the bottom of its channel, would, now that it is released, be set up again. The glacier would thus descend with differential motions, horizontally and vertically, of the ice at its bottom and sides, and a bodily motion of a central portion or core 4074 feet wide and 2037 feet deep.

It follows that, unless the channel of a glacier were more than 4074 feet wide and 2037 feet deep, it could not descend in it by its weight alone (its slope being that of the Mer de Glace)—and that the differential motion could not extend, as it actually does, for a considerable distance from the bottom and sides, unless the dimensions of the channel were greater than these. Now the dimensions of the imaginary glacier to which my former calculations referred (being those of the Mer de Glace at Les Ponts) were 1400 feet in width and 140 feet in depth, with a slope equal to that of the Mer de Glace; such a glacier, therefore, could not, according to the calculations I have *now* made, descend by its weight, as I found it could not by the wholly different calculations I made before. *This*, moreover, is to be observed—that, descending by its weight only, a glacier would have no *horizontal* differential motion whatever at its surface or within 2037 feet of it; whereas we know by the experiments of Agassiz on the Aar Glacier that the horizontal differential motion extends to the *very surface* of the glacier; for having placed rods of wood in borings from the surface and left them for a time, he found them all inclined in the direction of the descent\*.

But Mr. Mathews argues that a glacier descends by *bending*, and that in the act of bending the resistance to such shearing as, in addition to its bending, is necessary to its descent is diminished—and that, this being the case, its own weight becomes sufficient to cause it to descend†. The question is, then, does

\* See also Forbes's 'Occasional Papers,' pp. 173, 186.

† I hope I have stated Mr. Mathews's argument correctly, and the more so as I have mistaken some words in his former paper in the 'Alpine Journal' to imply a denial of any *shearing* of ice in the descent of glaciers. The words are (speaking of the bending of his ice-plank), "according to the views of Canon Moseley, *shearing must surely have been impossible.*" As I understood him to argue that a glacier bends like an ice-plank, I thought he must argue shearing to be also impossible in a glacier. I hold no views which justify either conclusion.



a glacier bend itself down? My answer to this question shall be founded on Mr. Mathews's own experiment. He placed a plank of ice  $2\frac{3}{8}$  inches thick between two supports six feet apart, and found that it sank in the middle until, in seven hours, under the influence of a *thaw* it had deflected seven inches. Of this experiment Mr. Mathews says, "I regard it as absolutely subversive of the Canon's theory that the descent of glaciers by their weight alone is a mechanical impossibility." These are strong words. I can only understand them by supposing that Mr. Mathews has other reasons than he has alleged for considering his experiment conclusive on the question at issue; for the conditions under which his ice-plank bent and the conditions under which a glacier descends are entirely different and have nothing in common. A glacier is nowhere an ice-plank, nor is it anywhere placed flatwise between two fixed supports and left to bend vertically with nothing beneath it to rest upon. If the Mer de Glace descends by bending, it must bend in the direction of its *length*, because it is in the direction of its length that it *descends*. To present a parallel to the descent of a glacier, Mr. Mathews should have placed his ice-plank between two supports, not flatwise, but endwise, and should have observed whether *then* it bent in the direction of its length. Moreover the Mer de Glace does not descend vertically, but on an inclined plane of  $4^{\circ} 52'$ . The ice-plank should then have been placed on a plane inclined at that slope, and it should have been observed whether when so placed it bent lengthwise. And even if it descended *then* by bending, it would not have descended under similar conditions to those under which the Mer de Glace descends—unless, placing a number of such ice-planks on one another, those nearer to the surface had descended faster than those further from it, notwithstanding that they were all frozen together. To apply Mr. Mathews's experiment to the case in hand, I will suppose his ice-plank to be lengthened from 6 feet to 1398 feet. It will then be about as *long*, I believe, as the Mer de Glace is wide at Les Ponts. I will further suppose it to become 22,600 feet thick, in which case it will be as thick as the Mer de Glace is long, measuring from the origin of the Glacier de Léchand to the Montanvert. This colossal ice-plank, 1398 feet long and 22,600 feet thick (or deep), I will then suppose to be laid down in a uniform rectangular channel at a slope of  $4^{\circ} 52'$ ; and, the sides and bottom of the channel being perfectly smooth, I will suppose the ice-plank or glacier not to be frozen to them, but prevented from slipping by two stops projecting a short distance from the opposite sides of the channel at the end of the glacier. Now I apprehend that, according to Mr. Mathews's theory, this glacier would, by its weight only, *bend* in the

direction of what has been called its thickness or depth (but which, now that it is laid down in the channel of a glacier, must be called its length), and that, in some way which has not been explained, it would bend *continually* more and more and so descend. For if it would not do so under these conditions, then *à fortiori* it would not do so under the actual conditions of a glacier, in which the stops—instead of being placed at the end only, and the sides and bottom being elsewhere without resistance—are distributed over the whole surface of the channel, sides, and bottom. Now it is my object to show that this imaginary glacier would not *bend* by its own weight only in the direction of its length under these conditions, and therefore *à fortiori* that it would not descend by bending under the conditions of an actual glacier.

For the sake of argument, I will suppose that the deflection of my imaginary glacier along its sloping channel in the direction of its length, and that of Mr. Mathews's ice-plank in the vertical direction of its thickness, are neither of them carried beyond what is called the elastic limit, so that neither of them takes a *set* in the act of deflecting. This being supposed, it is possible to determine by well-known formulæ what ratio the bendings or deflections of the two masses of ice would have to one another. It will, I hope, put no great mathematical strain on my readers to follow the investigation of this ratio, which I have subjoined in a note\*. It results from it that the bending downwards of the glacier will not be  $\cdot 019122$ , or not the fiftieth part of the bending of the ice-plank.

When the bending of a body is carried beyond certain limits, known as the limits of its elasticity, it takes a *set*. The ice-plank

\* If  $D$  represent the deflection, by its weight alone, of a solid having a rectangular cross section whose depth is  $c$ , and the distance of the two points on which it is supported  $a$ , and its breadth  $b$ , and if  $w$  be the weight of a cubic unit of it, and  $E$  its modulus of elasticity, and if it be borne in mind that the weight of such a mass produces the same effect in causing it to deflect, as five eighths of its weight would do if collected in its centre, then (see Morin, vide *Mémoire de Mécanique Pratique*, p. 481, ed. 5, or 'Mechanical Principles of Engineering,' by the author of this paper, p. 510, ed. 2)

$$D = \frac{a^3 \left( \frac{5}{8} \omega a b c \right)}{48 E \left( \frac{1}{12} b c^3 \right)} = \left( \frac{5 \omega}{32 E} \right) \frac{a^4}{c^2}.$$

But if, instead of being placed vertically, the solid had been laid upon a smooth incline whose inclination was  $\iota$ , the pressure of each cubic unit of its mass tending to cause it to deflect would have become  $\omega \sin \iota$  instead of  $\omega$ . In this case, therefore,

$$D = \left( \frac{5 \omega}{32 E} \right) \frac{a^4}{c} \sin \iota;$$

whence it follows that in respect of different solids so placed, having equal

does so. But it is in a high degree improbable that the glacier is carried beyond the limits of its elasticity whose bend is not the fiftieth part of that of the ice-plank. And this high improbability becomes, I think, an impossibility when it is considered that each infinitely thin horizontal stratum of the glacier must (to descend as an actual glacier does) bend further than the one below it, and in doing so overcome the resistance to shearing of the subjacent stratum to which it is frozen—so that to the resistance to its bending bodily must be added the resistances to the differential bendings of its superimposed strata.

Thus, from whatever point of view the question of the descent of glaciers is looked at, in the light of exact science, it is seen to be impossible they should descend by their weights only. As, however, it is possible that Mr. Mathews, taking his stand on the strong ground of popular science, may refuse to be convinced by mathematical reasoning even so elementary as that which I have here been using, I will submit to him the following considerations.

He argues that because an ice-plank bends vertically *flatwise* by its own weight, and takes a *set* when placed between two supports, therefore the Mer de Glace, when placed on a slope of  $4^{\circ} 52'$ , bends *lengthwise* by its own weight. He has nothing, so far as appears from his papers, but *that fact* to bring him to that *conclusion*—nothing to bridge over the intervening space.

Now if a plank of *lead*, long enough in relation to its thickness, were placed flatwise between two supports it would certainly bend and take a *set*. That is a fact strictly analogous to Mr. Mathews's fact of the ice-plank. According to him, therefore, if the Mer de Glace were of lead instead of ice it would descend by its weight as a glacier does.

But if the argument is good for ice and also for lead it is good also for iron. For if a plank of wrought iron thin enough, with reference to its length, were placed flatwise between two supports, it would bend by its weight and take a *set*. It follows,

moduli of elasticity and equal specific gravities,

$$D \propto \frac{a^4}{c^2} \sin \iota.$$

Now in the supposed glacier  $a=1398$  feet,  $c=22,600$  feet,  $\iota=4^{\circ} 52'$ .

In Mr. Mathews's ice-plank,  $a=6$  feet,  $c=0.1979$  feet,  $\iota=90^{\circ}$ .

If therefore  $D_1$  be the deflection of the glacier, and  $D_2$  that of the ice-plank,

$$\frac{D_1}{D_2} = \frac{1338^4 \sin 4^{\circ} 52'}{22600^2 \cdot 6^4 \cdot .1979^2 \sin 90^{\circ}} = .019122.$$



therefore, by Mr. Mathews's argument, that the Mer de Glace would descend by its weight only, if it were of iron instead of ice, and that it would descend with a differential motion.

Every body knows how much more easily a rod is bent than sheared across. The question "why this happens"—what are those molecular displacements lengthwise and crosswise which accompany this change of form, and what are the forces which oppose themselves to it—is one of the most subtle in physics. But *little* is to be understood of it by the light of the highest mathematics, and *nothing* without it. I will not invite Mr. Mathews to accompany me into that region. The question what would happen if a glacier bent itself down its channel does not indeed touch my argument, because I have shown that a glacier does *not* bend itself down. There is, however, an *inference* Mr. Mathews has drawn from the bending of his ice-plank which does touch my argument. The cross section of his ice-plank measured, he says, 12 square inches. If it were sheared across as ice was sheared in my experiments, it would, at 75 lbs. per square inch shearing-resistance, require 700 lbs. to shear it. But the entire weight of the ice-plank was only 32 lbs., and yet it deflected with this weight and therefore also sheared. Mr. Mathews is curious to see how I shall explain this fact to him. His explanation is, that instead of the unit of shear in ice being not less than 75 lb. per square inch, as I found it to be by direct experiment, it is shown indirectly by this experiment of his to be less than  $1\frac{1}{3}$  lb., probably much less.

Now I will suggest to Mr. Mathews a parallel experiment and a parallel explanation. If a bar of wrought iron 1 inch square and 20 feet long were supported at its extremities, it would *bend* by its weight alone, and would therefore shear. Now the weight of such a rod would be about 67 lbs. According to Mr. Mathews's explanation in the case of the ice-plank, the unit of shear in wrought iron should therefore be 67 lbs. per square inch. It is actually 50,000 lbs.

I think I have not left unanswered any question raised by Mr. Mathews's first paper.

With reference to his second paper, I have only to bear testimony to the value which I attach to his observations and those of Mr. Reilly on the Surface-motions of Glaciers. The exceedingly small differences of velocity of points exceedingly near to one another of which he speaks, following Mr. Ball, is no argument against the results at which I have arrived. Those results suppose *infinitely* small differences of velocity at points infinitely near to one another, and they aggregate the *work* done under those infinitesimally small differences of motion. The aggregate of an infinite number of infinitely small things may

be finite. From the *second* of the five propositions into which he divides my argument I entirely dissent. I do not hold the opinion (nor have I anywhere said that I do), that "the absolute motion of any point in the surface of a glacier is proportional to its distance from the nearest side and to its height from the bottom of the channel."

The want of uniformity from centre to side in the surface-motion of the glacier which Mr. Mathews and Mr. Reilly observed could not but have been due to irregularities in the bottom of its channel, or in its sides, or in its direction or slope. It has apparently escaped Mr. Mathews's recollection that I reason of an imaginary glacier of homogeneous ice with no irregularities of the bottom or sides of its channel or in its direction or slope, whose surface-motion from centre to side could not but be uniform as to the two sides. If it be true that a glacier, such as *this imaginary one*, could not descend by its weight only, the conclusion is irresistible, that an actual glacier, whose channel is irregular as to its surface, direction, and slope, *could not*.

I have endeavoured to make intelligible otherwise than in the language of mathematical science a mathematical argument. On reading what I have written I cannot congratulate myself on my success. Neither party to an argument is likely to be convinced by the reasoning of the other, nor is any valuable result likely to be arrived at by them, when they do not argue in the same language.

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VI. *On the Spectroscopic Observation of the Rotation of the Sun, and a new Reversion-Spectroscope.* By F. ZÖLLNER\*.

IN compliance with a friendly invitation, I repaired, at the Whitsuntide vacation, to Bothkamp, near Kiel, in order to take in hand, at Chamberlain von Bülow's splendidly endowed private observatory there, those investigations on which I had the honour, two years since, to make some preliminary communications to the Society, when exhibiting my reversion-spectroscope.

The large refractor, by Schröder of Hamburg, set up in Bothkamp, and furnished with an excellent clockwork, is not only, after the Pulkowa refractor, the largest on the Continent, but probably takes the first rank among all the refractors of equal size by its high optical and mechanical finish. Dr. H. C. Vogel (the director of the observatory) and Dr. Lohse, as assistant, in accord with the scientific intention of the founder, have

\* Translated from a separate impression, communicated by the Author, from the *Berichte der Königl. Sächsischen Gesellschaft der Wissenschaften, Math.-Phys. Classe*, for July 1, 1871.

set themselves the problem of turning to account in the service of *astrophysics* the means so very liberally placed at their disposal. The following communications on the spectroscopic observation of the solar rotation, and Dr. Vogel's investigations on the aurora borealis\*, may be regarded as the firstfruits of these meritorious endeavours.

For the object intended, the reversion-spectroscope had received some alterations, which had reference partly to its own construction, and partly to the manner of fixing on the Bothkamp refractor. These were not quite finished in time for my departure; so that I was obliged to have the instrument sent after me. It arrived at Bothkamp on the 21st of May, and was immediately attached to the refractor. Unfortunately, during the three days which still remained for my stay there, the weather was unfavourable. Only in the evening of the 2nd of June did a break in the clouds when the sun was near the horizon permit at least one trial to be made. While I looked through the spectroscope, Vogel, with the aid of the finder, directed the refractor upon various parts of the sun's disk. I observed a small displacement of the sodium-lines, which had been brought to coincidence in the two spectra, and, without knowing any thing of the position of the slit on the solar disk, always answered Vogel's questions about the direction of the displacement in a manner corresponding to the situation. But after the lapse of a few minutes the sky again became covered; so that, substantially, I returned without having accomplished my purpose, and left my instrument with MM. Vogel and Lohse for further investigations under more favourable conditions.

To my joy, after a few days I received a letter, dated June 9, 1871, in which Dr. Vogel gives me,

"Today, as briefly as possible, the intelligence that we (Dr. Lohse and I) yesterday positively and repeatedly saw the displacement of the lines by the rotation of the sun, by means of the reversion-spectroscope. With the present arrangement of the apparatus, it is hardly possible to execute measurements with any certainty; above all, the two spectra must be perfectly equal in brightness, and the slit must be as fine as it is anyhow possible to make it, since in such delicate measurements the cross lines are too coarse."

A second letter, dated June 14, contains the following:—

"Since with your spectrum-apparatus (as I have already informed you) the displacement of the lines by the rotation of the sun was seen, with my own I have not only seen a displacement, but estimated its magnitude, by using five strongly dispersing prisms arranged in a circle, having previously directed them

\* *Berichte der Kön. Sächs. Ges., l. c.*



upon the region near F. I see the fine line behind F (distance 1·5 ten-millionth of a millimetre; see Angström's Atlas) about half as far from the line F as the distance between the sodium-lines amounts to in your spectroscope. If necessary, one twentieth of this distance can be estimated. All the estimates by Dr. Lohse and myself give not quite 0·1 of a ten-millionth of a millimetre, which would correspond to a difference of velocity of the equatorial limbs of about 0·7 of a mile, while in reality the rotation amounts to  $2 \times 0\cdot27$ , therefore 0·54 mile. When the telescope was directed to the north or the south point of the sun, there was no displacement, therefore *no flexure of the individual parts of the apparatus during the observation*. Accordingly the proof of the rotation of the sun by means of the spectroscope is to be considered *certain*. I will, besides, repeat the experiments with still higher magnifying-power.

"Further, a second, not less interesting fact. In the spectra of some nebulae (planetary), which according to other observers show only three lines, I have been able to find some additional lines; of these it is in the highest degree probable that one, quite distinctly seen by Lohse and myself, is coincident with the *brightest* line of the aurora borealis. This would give rise to remarkable conclusions."

A more detailed description of these and subsequent observations is given by Dr. Vogel in the following:—

#### Observations.

"1871, June 9. With the aid of your reversion-spectroscope the displacement of the lines by the rotation of the sun was decidedly seen by Dr. Lohse and myself. The experiments were made in the following manner:—

"While the spectroscope, with the aid of the clockwork, was directed to one (the preceding) limb of the sun, the coincidence of the D lines in the two spectra lying one over the other was effected with the utmost sharpness possible; the clockwork was then stopped and the moment of the egress of the second (following) limb observed. The non-coincidence of the D lines at the second limb of the sun was distinctly perceptible in *all* the very often repeated observations.

"June 10. We repeated the experiments with our very strongly dispersing spectroscope by Schröder, which consists of five direct-vision prisms and as many prisms of very heavy glass arranged in a circle. The observing-telescope magnified nine times. In the focus was a fine steel point, which was put to the line F, or to a very fine line a little more refrangible than F (wave-length, according to Angström, 4859·17), while the spectroscope was directed to one limb of the sun. The point no

longer covered the line when the light of portions of the other limb fell on the slit. In order to avoid flexures, the precaution was taken of fixing the telescope and letting the image of the sun pass over the slit by the diurnal motion. With direction to the vicinity of the north or the south pole of the sun (where no displacement was to be expected), the coincidence of point and spectrum-line remained perfectly unaltered; and this was a guarantee that any alterations that, with a little movement of the refractor, may happen to take place in the spectrum-apparatus are so small as to be without influence on the observations.

"June 11. The observations were made in the same manner as on the previous day. We endeavoured, by numerous estimations, to fix the magnitude of the displacement of the lines towards the point in the focus of the telescope, taking as unit the distance between two lines near together in the spectrum. Our accounts varied between 0.010 and 0.015 millionth of a millimetre—which would give, for the motion of a point in the solar equator, a velocity of 0.42 mile in a second.

"June 15. The observations were carried out as before; only, instead of the point in the observing-telescope, I had very fine cross-threads introduced. A twenty-four times magnifying-power could be used with advantage; with it Fraunhofer's lines appeared extremely sharp. In the vicinity of the line F and the group *b* I made preliminary estimations, which gave 0.008 millionth of a millimetre for the amount of the displacement, from which 0.35 mile would result as the velocity of an equatorial point. It is a striking fact that the observations always give a greater velocity than that calculated from the known period of revolution of the sun; yet it would be hazardous to deduce any conclusion whatever therefrom, since, on the one hand, the estimates are very uncertain, and, on the other, the wave-lengths of the lines in the solar spectrum are not so accurately determined that the uncertainty, in comparison with the amount to be determined, is a vanishing quantity. Thus much only results from all the observations—that a displacement of the lines by the rotation of the sun may be regarded as certainly proved."

As an appendix to these results (which evidently promise for the reversion-spectroscope a very extended application in future for quantitative determinations in spectrum-analytical investigations), I take leave to communicate the construction of a new and considerably simplified reversion-spectroscope. I have already alluded to it, in the description of the one before mentioned\*; but since then I have been so convinced of its practical utility, that I am sure the principle can be applied with great

\* *Berichte der K. Sächs. Gesellsch. d. Wiss. math.-phys. Classe*, Feb. 6, 1869, p. 73.

facility for *all* spectroscopic investigations, without its being necessary, as hitherto, to use cross-threads, points, or illuminated objects for the determination of the relations of position of the lines.

The requisite disposition of the observing-tube of *any spectro-scope* can be secured in two ways, namely:—1, by the reversion-objective; 2, by the reversion-ocular.

### 1. *Description of the Reversion-Objective.*

The objective of the observing-tube is divided diametrically; and the two halves, by means of screws, can be shifted *only perpendicularly* to the line of division—that is, brought nearer together or their mutual distance increased. In front of one of these two halves a rectangular reflection-prism is placed, moveable, in such wise that the hypotenuse-surface is perpendicular to the plane which is parallel with the line of division, and, in the normal arrangement, parallel with the optic axis of the telescope. When any object is viewed through a telescope provided with such an objective, in a direction perpendicular to the line of division of the objective it appears double. On the one hand, it depends on the dimensions of the object, on the other on the distance between the two halves of the objective, whether the two components of the double image exactly touch, or are superposed, or are separated the one from the other. But, at the same time, the one of the two components the rays of which have passed through the reflection-prism is inverted relatively to an axis perpendicular to the line of division.

Hence, if the observing-tube of any spectrum-apparatus be replaced by a telescope with such an arrangement, and the latter be placed so that the refracting edges of the reflecting and dispersing prisms are parallel, two contiguous spectra with the series of colours in opposite directions will be obtained when the distance between the two halves of the objective is regulated accordingly. As with non-parallel rays the divergence or convergence is altered by total reflection, in such cases a moveable half of a lens must be placed in the observing-tube in order to restore equal focal distance of the two halves of the objective.

By changing the direction of the optic axis of the observing-tube in the manner which is usual for the observation of different parts of the spectrum in the middle of the field of view, one sees the lines of the two spectra move through the field in opposite directions, and can thus, by reading off the angle of inclination, when *the same* lines in the two spectra coincide, gather their position—exactly as when sights are used in the field, *but with double the amount of dispersion, and with the greater accuracy resulting from the principle of double images.* Moreover differential



*determinations can be made by a very delicate movement of the reflecting-prism.*

## 2. Description of the Reversion-Ocular.

The object of the reversion-ocular, and the principle of its operation, are the same as those of the reversion-objective; but while the latter presupposes a divided objective, in the employment of the reversion-ocular this is not the case.

That is to say, it contains the moveable reflection-prism, on a correspondingly diminished scale, close in front of the collective lens of the ocular, so that the field appears half covered by this prism, and thus the two spectra are moveable in juxtaposition in opposite directions. Since no parallel rays fall on the reflection-prism, the corresponding correction of the focal distance is provided for by the moveable half of a concave lens in the part of the ocular not covered by the prism.

With the reversion-ocular the contact of the two spectra is far less sharply defined than with the reversion-objective. This inconvenience, however, can be partially obviated by the employment of a cylindrical lens in front of the ocular, by which the lines are lengthened, and at the same time the dark divisions are weakened. I make bold to propose generally such an employment of cylindrical lenses immediately in front of the cover of the ocular wherever cross lines arising from dust or other inequalities of the slit produce disturbance in delicate measurements. These lines are hereby completely neutralized, and, indeed, vanish when their thickness is not too great, while the lines of the spectrum, perpendicular to them, lose nothing of their sharpness.

## VII. *On the Solutions of Three Problems in the Calculus of Variations, in reply to Mr. Todhunter. By Professor CHALLIS, M.A., LL.D., F.R.S., F.R.A.S.\**

**I** UNDERSTOOD the problem which is the subject of Mr. Todhunter's remarks in the December Number in the only sense that the terms in which it was enunciated admit of, and therefore I do not consider myself accountable for any misapprehension of what was intended. The enunciation, as given in the 'History of the Calculus of Variations' (p. 427) and repeated in the above-mentioned remarks, is as follows:—"Required to connect two fixed points by a curve of given length so that the area bounded by the curve, the ordinates of the fixed points, and the axis of abscissæ shall be a maximum." Accord-

Communicated by the Author.

ingly I supposed that the line joining the two ordinates was to be a *curve*; and as a portion of a circle fulfilled all the conditions of the question, I did not imagine that the joining line could be made up of a curve and one or more straight lines, nor did I gather from the enunciation that "it was intended the curve should be confined between indefinite straight lines coinciding in position with the extreme ordinates." On turning, however, to Legendre's memoir, I find that after taking account of the problem as above proposed, he adds, "But if it be required that the surface  $ABCD$  be absolutely contained between the two parallels  $AC$ ,  $BD$ , &c.," thus introducing a new condition which requires the discussion of two additional problems. This is intelligible enough. But in the 'History' there is no intimation that such a limitation was expressly made by Legendre.

It seems to me that the investigations in Stegmann's work (pp. 171-180), and those in Mr. Todhunter's (pp. 427-430), are liable to be misunderstood from the circumstance that the authors discuss under one enunciation *three* problems requiring different enunciations and different processes of solution. These problems might be proposed as follows:—

Required to construct a figure bounded by the axis of abscissæ, two ordinates drawn at given points of the axis, and a curve joining their extremities, so that its contour shall be of given length and its area a maximum:

(I.) When the length of each ordinate is given.

(II.) When the length of one of the ordinates is given.

(III.) When the length of neither of the ordinates is given.

The first of these three problems accords with that expressed by Mr. Todhunter's enunciation, the length of the curve being implicitly given. In the other two the lengths of the curves and the values of the extreme ordinates that are not given have to be determined by the solutions of the problems. In all the cases the curve is a portion of a circle. In Problem (I.) the circle may or may not extend in the direction of the axis of abscissæ beyond one or both of the given extreme ordinates; in Problem (II.) it may or may not extend beyond the given extreme ordinate, but cannot extend beyond the other; in Problem (III.) it can extend beyond neither ordinate. The limits of the given lengths of contour, for which the curve is wholly contained within the extreme ordinates produced, are readily deducible from the respective solutions. Mr. Todhunter says in two instances that a certain result is not "satisfactory," and that the problem requires to be "modified." These expressions, the meaning of which I did not apprehend, will be seen to be inapplicable if it be understood that there are, in fact, three problems, the separate solution of each of which is perfectly satis-

factory. The different solutions do not require any analytical processes different from those employed in the investigations above referred to.

I partly dissent from the solution of Problem (I.) by polar coordinates, as given by Mr. Todhunter in pp. 68–70 of his ‘History,’ on the ground that the process of integrating with respect to  $\theta$  from 0 to  $2\pi$  depends on assuming the position of the pole to be within the closed curve, whereas it is a general rule that the nature of a curve which is required to fulfil the condition of a maximum or minimum admits of being determined independently of the arbitrary position of the origin of coordinates. This might be done in the present instance by adopting the principles expressed by the two Lemmas I have announced in the *Philosophical Magazine* for July, p. 28.

The method of integration by the intervention of an evolute, employed in the solutions of the second and third problems discussed in the July Number, is so entirely new that I might have anticipated that its application in the *Calculus of Variations* would be called in question. I shall be glad to give attention to any arguments Mr. Todhunter may bring against the results to which the method has conducted in those instances. Although the two problems have hitherto received only discontinuous solutions, I venture to assert that, whether or not I have been successful, they must also admit of continuous solutions such as those which I profess to have obtained. It might be worth while to compare the content of a maximum solid given by the discontinuous solution with that of the solid deducible from the same data by the new method, for the purpose of determining which is the greater; but at present I have not leisure for the requisite numerical calculation.

Cambridge, December 8, 1871.

### VIII. *Proceedings of Learned Societies.*

#### ROYAL SOCIETY.

[Continued from vol. xlii. p. 385.]

February 23, 1871.—William Spottiswoode, M.A., Treasurer and Vice-President, in the Chair.

THE following communication was read:—

“On the Thermo-electric Action of Metals and Liquids.” By George Gore, F.R.S.

It is well known that the degree of rapidity with which a metal immersed in an acid, alkaline, or saline liquid is corroded varies considerably with the temperature, and that the speed of corrosion usually increases with the heat; also a few experiments have been published (Gmelin’s ‘*Handbook of Chemistry*,’ vol. i. p. 375) show-



ing that changes of electrical state occur in metals under such circumstances; but a further examination of the relations of the temperature and chemical change to the electrical state has not, that I am aware, yet been made.

In an investigation on the development of electric currents by unequally heated metals in liquids (*Phil. Mag.* 1857, vol. xiii. p. 1), I found that hot platinum was electro-negative to cold platinum in liquids of acid reaction, and positive to it in alkaline ones, provided in all cases chemical action was completely or sufficiently excluded. In the present experiments I have endeavoured to ascertain what electrical changes are produced in cases where chemical action more freely occurs, and I have therefore employed not platinum plates, but plates composed of a metal (copper) which is more easily corroded.

To effect the object I had in view, I used the apparatus shown in section in fig. 1, and in perspective, with its wooden support, in fig. 2.

Fig. 1.

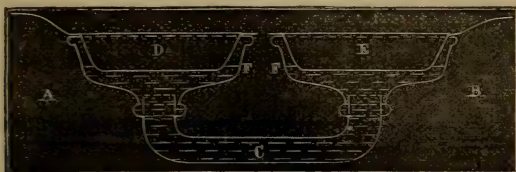
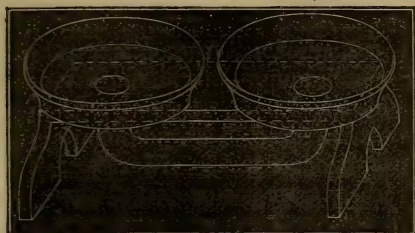


Fig. 2.



A and B, fig. 1, are two open thin glass dishes,  $6\frac{1}{2}$  inches diameter, and  $1\frac{1}{2}$  inch deep, with open necks. The dishes are joined together, water-tight, by a bent glass tube, C, about 1 inch in diameter; and the whole arrangement is securely fixed upon a wooden frame or stand, so that it may be at once placed in an exactly horizontal position, or inverted to pour out its contents. D and E are two dishes of sheet copper of moderate thickness, made from contiguous portions of a sheet of metal to ensure electrical homogeneity in the experiments. Wires of similar metals are attached to the dishes for the purpose of connexion with a galvanometer. A galvanometer, containing about 180 turns of moderately fine copper wire, is sufficiently sensitive for the experiments. The outside of the metal dishes must be made perfectly clean and bright immediately before each experiment.

In using the apparatus it is first set exactly horizontal, and a known and measured volume of the clear liquid to be examined, at the tem-

pérature of the atmosphere and sufficient to fill it to the line F F, is poured in; the metal dishes are then steadily placed in the glass vessels and connected with the galvanometer, taking care that no air-bubbles remain beneath them. As soon as the galvanometer-needles have settled at zero, one of the dishes is quickly filled with boiling water, and the directions and amounts of the temporary and permanent deflections noted.

The following are Tables of results obtained with various liquids, the solutions being diluted in each case to a specified measure by addition of distilled water. Those of the experiments in which 20 ounces of liquid were used, were nearly all of them made with an apparatus in which the connecting-tube C was of somewhat less diameter; and the deflections obtained by that apparatus were less in extent than those obtained with the "new apparatus," because in the latter the conduction-resistance was somewhat less. The values of the deflections given in the Tables are in all cases those of the *temporary* ones; and the liquid used for diluting the solutions was in all cases *water*.

#### *Pure Nitric Acid.*

No.	Ounces of strong acid diluted to 20 ozs. with water.	Value of Deflection.	
1.	.... $\frac{1}{16}$ ....	·0045	The hot plate was negative and much acted upon, especially with the stronger mixtures. With the stronger mixtures a little gas was evolved at 60° Fahr., and a large amount directly the heat was applied.
2.	.... $\frac{1}{8}$ ....	·0012	
3.	.... $\frac{1}{4}$ ....	·0039	
4.	.... $\frac{1}{2}$ ....	·0497	
5.	.... 1 ....	·1177	
6.	.... 2 ....	·0356	
7.	.... 3 ....	·0578	
8.	.... 4 ....	·4954	

#### *Pure Hydrochloric Acid.*

No.	Ounces of strong acid diluted to 20 ozs.	Value of Deflection.	
1.	.... $\frac{1}{16}$ ....	·0064	The hot plate was positive. The amount of stain upon the hot plate was very small, and was in the form of a dark line at the edge of the liquid.
2.	.... $\frac{1}{8}$ ....	·0330	
3.	.... $\frac{1}{4}$ ....	·1112	
4.	.... $\frac{1}{2}$ ....	·2854	
5.	.... 1 ....	·5731	
6.	.... 2 ....	2·0446	

#### *Chloric Acid.*

No.	Ounces of strong acid diluted to 20 ozs.	Value of Deflection.	
1.	.... $\frac{1}{16}$ ....	·0002	The hot plate was negative, and was but little acted upon. With the strongest mixture, the liquid in contact with the hot plate soon became green.
2.	.... $\frac{1}{8}$ ....	·0016	
3.	.... $\frac{1}{4}$ ....	·0040	
4.	.... $\frac{1}{2}$ ....	·0287	
5.	.... 1 ....	·1234	
6.	.... 2 ....	·2005	

#### *Hydrobromic Acid.*

No.	Weak acid diluted to 10 ozs.	Value of Deflection.	
1.	.... $\frac{1}{2}$ ....	·0149	Hot plate negative. Both plates much stained, the cold one the most so.
2.	.... 1 ....	·0647	

*Crystallized Boracic Acid.*

The hot plate was positive. A series of six solutions was employed, containing from 50 grains to nearly 400 grains in 20 ounces by measure of water, the strongest being a saturated solution. The currents obtained were extremely feeble, and the plates were not tarnished.

*Aqueous Hydrofluosilicic Acid.*

Value of deflection ·1488. The hot plate was negative, and became a little tarnished.

*Pure Sulphuric Acid.*

No.	Ounces of strong acid diluted to 20 ozs.		Value of Deflection.	
1.	.... $\frac{1}{18}$	....	·0077	The hot plate was negative, and the plates were but little tarnished.
2.	.... $\frac{1}{8}$	....	·0161	
3.	.... $\frac{1}{4}$	....	·0418	
4.	.... $\frac{1}{2}$	....	·0878	
5.	.... 1	....	·1044	
6.	.... 2	....	·0327	
7.	.... 4	....	·0037	
8.	.... 8	....	·0319	

*Pure Phosphoric Acid, solid.*

No.	Grains of the glacial acid diluted to 20 ozs.		Value of Deflection.	
1.	.... 100	....	·00005	The hot plate was positive, and the plates were not vi- sibly tarnished.
2.	.... 200	....	·00005	
3.	.... 400	....	·00040	
4.	.... 800	....	·00060	
5.	.... 1600	....	·00370	

*Chloride of Copper (Basic; solution filtered).*

No.	Grains of the salt diluted to 12 ozs.		Value of Deflection.	
1.	.... 100	....	·0025	Hot copper positive. " negative.
2.	.... 500	....	·0198	
3.	.... 1000	....	·0025	

Much action on both plates, especially the hot one, and basic chloride of copper formed.

*Chlorate of Copper.*

In a moderately strong solution of this salt, which had been digested with an excess of carbonate of copper and filtered, the hot plate was negative; value of deflection ·2997. Both plates were acted upon, but the hot one the most. The liquid had a feebly acid reaction.

*Sulphate of Copper.*

No.	Grains diluted to 20 ozs.		Value of Deflection.	
1.	.... 249·5	....	·0016	Hot copper negative.
2.	.... 499·0	....	·0081	Liquid acid.



*Chloride of Cobalt.*

No.	Ounces diluted to 12 ozs.		Value of Deflection.	
1.	....	$\frac{1}{2}$ oz. saturated solution	·1247	} Hot copper posi- tive. Liquid acid. No stains, except slightly at edge of hot liquid.
2.	....	3 ozs.       "       "	1·2726	

*Protosulphate of Iron.*

No.	Grains diluted to 12 ozs.		Value of Deflection.	
1.	....	200       ....	·0198	} Hot copper negative. No stains on either plate.
2.	....	800       ....	·0794	

*Chloride of Manganese.*

No.	Grains diluted to 12 ozs.		Value of Deflection.	
1.	....	100       ....	·0436	} Hot copper positive. No stains. Liquid neutral, or very faintly acid.
2.	....	1000       ....	1·0995	

*Chromic Acid.*

No.	Ounces of strong solution diluted to 12 ozs.		Value of Deflection.	
1.	....	$\frac{1}{2}$ ....	·0987	} Hot copper positive. Both plates much acted upon, apparently the cold one the most.
2.	....	2       ....	18·8034	

*Chloride of Chromium.*

No.	Ounces of strong solution diluted to 10 ozs.		Value of Deflection.	
1.	....	$\frac{1}{2}$ ....	·0293	} Hot copper positive. The plates appeared unaffected. The solu- tion was weakly acid to test-paper.
	....	1       ....	·1319	

*Nitrate of Lead.*

No.	Grains.		Value of Deflection.	
1.	....	100 diluted to 12 ozs.	·0009	} Hot plate negative, No stains. Liquid extremely faintly acid.
2.	....	Saturated solution (undiluted)	·0259	

*Sulphate of Zinc.*

No.	Grains diluted to 20 ozs.		Value of Deflection.	
1.	....	287       ....	·0080	} Hot copper negative. } Liquid positive.       4. neutral.
2.	....	574       ....	·0048	

*Sulphate of Magnesium.*

No.	Grains.	Value of Deflection.
1. ....	50 diluted to 20 ozs.	·0001
2. ....	100   "   "	·0002
3. ....	200   "   "	·0006
4. ....	246   "   "	·0100
5. ....	400   "   "	·0036
6. ....	492   "   "	·0208
7. ....	800   "   "	·0130
8. ....	1000   "   "	·0228
9. ....	2000   "   "	·0607
10. ....	Saturated solution (undiluted).	·0463

Hot copper positive, and liquid neutral.

*Chloride of Calcium.*

1400 grains diluted to 20 ounces. Value of deflection ·2935.  
Hot copper positive. Liquid neutral. Hardly any stain, most on hot plate.

*Nitrate of Strontium.*

No.	Grains diluted to 12 ozs.	Value of Deflection.
1. ....	100   "   "	·0368
2. ....	1000   "   "	·2321

Hot plate positive. No stain. Liquid neutral.

*Chloride of Strontium.*

No.	Grains diluted to 20 ozs.	Value of Deflection.
1. ....	400   "   "	·2088
2. ....	800   "   "	·3277
3. ....	1600   "   "	·6671
4. ....	3200   "   "	·6654

Hot copper positive. Liquid neutral. Hardly any chemical action, most on hot plate.

*Nitrate of Barium.*

No.	Grains.	Value of Deflection.
1. ....	100 diluted to 20 ozs.	·0016
2. ....	Saturated solution (undiluted)	·1170

Hot copper positive. No stain. Liquid neutral.

*Chloride of Barium.*

No.	Grains.	Value of Deflection.
1. ....	50 diluted to 20 ozs.	·0016
2. ....	100   "   "	·0049
3. ....	200   "   "	·0145
4. ....	244   "   "	·0257
5. ....	400   "   "	·0214
6. ....	488   "   "	·0259
7. ....	800   "   "	·0234
8. ....	1600   "   "	·0802
9. ....	Saturated solution (undiluted).	·3142

Hot copper positive. Liquid neutral. A little copper was dissolved by the solution.

*Nitrate of Sodium.*

No.	Grains diluted to 20 ozs.	Value of Deflection.	
1. ....	85 .....	·0025	Hot copper negative. { Liquid
2. ....	255 .....	·0328	} positive. { neutral.
3. ....	510 .....	·1015	

*Chloride of Sodium.*

No.	Grains.	Value of Deflection.	
1. ....	12·5 diluted to 20 ozs.	·0001	
2. ....	25 " "	·0012	
3. ....	50 " "	·0081	
4. ....	75 " "	·0153	
5. ....	100 " "	·0293	
6. ....	150 " "	·0512	Hot copper positive.
7. ....	200 " "	·0819	Tarnish at edge of hot
8. ....	250 " "	·1016	liquid, especially with
9. ....	300 " "	·1160	the strongest solu-
10. ....	400 " "	·1906	tions.
11. ....	500 " "	·2241	
12. ....	600 " "	·2708	
13. ....	800 " "	·3473	
14. ....	1000 " "	·4884	
15. ....	2000 " "	·4237	
16. ....	Saturated solution (undiluted)	·3479	

*Iodide of Sodium.*

No.	Grains diluted to 12 ozs.	Value of Deflection.	
1. ....	100 ....	·0100	Hot copper negative. { Liquid alkaline.
2. ....	1000 ....	·0819	} positive. { No stains.

*Carbonate of Sodium.*

No.	Grains diluted to 20 ozs.	Value of Deflection.	
1. ....	286 .....	·0468	Hot copper positive.
2. ....	572 .....	·1673	Liquid alkaline.

*Biborate of Sodium.*

Six ounces of a saturated solution diluted to 12 ounces. Hot copper was positive; value of deflection ·0452.

*Sulphate of Sodium.*

No.	Grains diluted to 20 ozs.	Value of Deflection.	
1. ....	50 .....	·0001	Hot copper negative. { Liquid
2. ....	100 .....	·0009	} positive. { neutral.
3. ....	200 .....	·0016	
4. ....	1000 .....	·0170	



*Phosphate of Sodium.*

No.	Grains diluted to 20 ozs.	Value of Deflection.	
1.	358	·0382	Hot copper positive.
2.	716	·0648	Liquid alkaline.

*Nitrate of Potassium.*

No.	Grains.	Value of Deflection.	
1.	50 diluted to 20 ozs.	·0025	
2.	100 " "	·0107	
3.	200 " "	·0259	Hot plate positive. No stains at all on the plates. Solution quite neutral.
4.	400 " "	·0647	
5.	800 " "	·1328	
6.	1600 " "	·2997	
7.	Saturated solution(undiluted)	·2852	

*Chloride of Potassium.*

No.	Grains.	Value of Deflection.	
1.	25 diluted to 20 ozs.	·0010	
2.	50 " "	·0064	
3.	100 " "	·0145	Hot plate positive, and became tarnished at the edge of the liquid, especi- ally with the stronger so- lutions. Traces of copper were found to have dis- solved. The solutions were neutral to test-paper.
4.	200 " "	·0442	
5.	300 " "	·0667	
6.	400 " "	·0882	
7.	500 " "	·1239	
8.	600 " "	·1396	
9.	800 " "	·1874	
10.	1000 " "	·2443	
11.	2000 " "	·6371	
12.	Saturated solution(undiluted)	·8439	

*Chlorate of Potassium.*

No.	Grains diluted to 20 ozs.	Value of Deflection.	
1.	122·5	·0054	Hot plate positive, and became tarnished. Solution neutral.
2.	245·0	·0453	

*Bromide of Potassium.*

No.	Grains diluted to 20 ozs.	Value of Deflection.	
1.	100	·0497	Hot plate negative. } No stains. } Liquid } positive. } neutral.
2.	500	·1080	
3.	1000	·3150	

*Iodide of Potassium.*

No.	Grains diluted to 12 ozs.	Value of Deflection.	
1.	100	·0259	Hot plate negative. } No stain. } Liquid } positive. } neutral.
2.	550	·0452	
3.	1000	·0159	

*Iodate of Potassium.*

No.	Grains.	Value of Deflection.	
1.	100 diluted to 12 ozs.	·0064	} Hot plate negative. Both plates much stained by formation of iodide of copper.
2.	Saturated solution (undiluted)	·0100	

*Acid Carbonate of Potassium.*

No.	Grains.	Value of Deflection.	
1.	50 diluted to 20 ozs.	·0049	} Hot plate positive. The liquid on evapo- ration was green with dissolved copper, Li- quid alkaline. Hot plate alone much tar- nished.
2.	100 " "	·0170	
3.	200 " "	·0497	
4.	400 " "	·0818	
5.	600 " "	·1329	
6.	800 " "	·1978	
7.	1000 " "	·2441	
8.	2000 " "	·4210	
9.	Saturated solution (undiluted)	·5451	

*Carbonate of Potassium.*

No.	Grains.	Value of Deflection.	
1.	50 diluted to 20 ozs.	·0122	} Hot plate positive.
2.	100 " "	·0382	
3.	200 " "	·1770	
4.	400 " "	·3719	
5.	800 " "	·7521	
6.	1600 " "	2·2400	
7.	2400 " "	3·7708	
8.	3200 " "	·4367	
9.	Saturated solution (undiluted)	·4031	

*Acid Sulphate of Potassium.*

Saturated solution (undiluted). Value of deflection ·1047, Hot plate negative.

*Bichromate of Potassium.*

No.	Grains diluted to 20 ozs.	Value of Deflection.	
1.	295	·0785	} Hot metal positive.
2.	590	·1544	

*Chrome Alum.*

No.	Grains diluted to 20 ozs.	Value of Deflection.	
1.	249·8	·0019	} Hot metal negative. Liquid of acid reac- tion.
2.	499·6	·0064	

*Aqueous Ammonia.*

Copper in a mixture of 4 ounces of water and 400 grains of aqueous ammonia at 180° Fahr. was electro-positive to copper in the same mixture at 60° Fahr.

*Nitrate of Ammonium.*

No.	Grains diluted to 20 ozs.	Value of Deflection.	
1. ....	80	0002	Hot plate negative. Acid reaction.
2. ....	240	0228	
3. ....	480	0590	

*Chloride of Ammonium.*

No.	Grains.	Value of Deflection.	
1. ....	25 diluted to 20 ozs.	0020	Hot copper positive. Solutions extremely faintly acid. Both plates tarnished by the stronger solution but the hot one the most so, and a little copper was dissolved.
2. ....	50     "     "	0029	
3. ....	100   "     "	0147	
4. ....	200   "     "	0647	
5. ....	400   "     "	1583	
6. ....	800   "     "	5551	
7. ....	1000   "     "	6744	
8. ....	1600   "     "	6258	
9. ....	2000   "     "	7479	
10. ....	Saturated solution (undiluted)	1210	

*Aqueous Hydrocyanic Acid.*

Scheele's strength. The hot plate was feebly positive. Value of deflection 0006.

*Cyanide of Potassium.*

No.	Grains diluted to 12 ozs.	Value of Deflection.	
1. ....	100	2854	Hot copper positive. Much gas evolved from the hot plate only in the strongest solution.
2. ....	1000	18164	

*Ferrocyanide of Potassium.*

No.	Grains diluted to 20 ozs.	Value of Deflection.	
1. ....	500	0136	Hot plate positive. Liquid feebly alka- line. Both plates be- came pink like new copper.
2. ....	1000	0045	

*Oxalic Acid.*

No.	Grains.	Value of Deflection.	
1. ....	25 diluted to 20 ozs.	0001	The hot plate was negative, and the plates were not tar- nished at all.
2. ....	50     "     "	0002	
3. ....	100   "     "	0006	
4. ....	200   "     "	0016	
5. ....	400   "     "	0064	
6. ....	Saturated solution (undiluted)	0070	

*Glacial Acetic Acid.*

The hot plate was negative. Seven solutions, containing from  $\frac{1}{8}$  ounce to 4 ounces by measure of the acid in 20 ounces by measure, gave only extremely feeble currents. The plates remained bright.



*Acetate of Sodium.*

No.	Grains diluted to 20 ozs.	Value of Deflection.
1. ....	50	·0016
2. ....	100	·0070
3. ....	200	·0198
4. ....	400	·0594
5. ....	800	·1328
6. ....	1600	·2202
7. ....	2000	·2850
8. ....	2727	·2997

Hot plate positive.  
Solution alkaline to  
test-paper.

*Acetate of Zinc.*

No.	Grains diluted to 20 ozs.	Value of Deflection.
1. ....	50	·0001
2. ....	100	·0006
3. ....	200	·0016
4. ....	400	·0025
5. ....	500	·0020
6. ....	800	·0020
7. ....	1000	·0012
8. ....	1600	·0004
9. ....	2000	·0001

Hot plate positive.

*Crystallized Tartaric Acid.*

The hot plate was negative. Eight different solutions, varying in strength from 50 to 3200 grains in 20 ounces by measure of the solution, were tried; but very feeble currents were obtained, and the plates were not tarnished.

*Crystallized Citric Acid.*

The hot plate was negative. With a series of seven solutions, varying in strength from 50 to 3200 grains in 20 ounces of liquid, more feeble results, even, than those with tartaric acid were obtained, and the plates were not tarnished. Probably, with this substance and with others where the resulting currents were very feeble, more distinct effects would be obtained by employing a galvanometer of much greater electrical resistance.

Several experiments similar to those already described were made with the apparatus shown in fig. 3. The apparatus consists of a glass beaker containing the liquid, and two platinum electrodes—A being a disk of platinum rivetted to a platinum wire enclosed by a glass tube, B, and C a platinum crucible (for receiving the boiling water) with a platinum wire rivetted to it.

*Experiment 1.*—With a solution of 100 grains of citric acid in 2 ounces of distilled water, the hot platinum cup was negative, the value of the temporary deflection being ·0007.

*Experiment 2.*—With 100 grains of tartaric acid in 2 ounces of water, the hot cup was negative, value of deflection ·0001.

*Experiment 3.*—With 100 grains of racemic acid in 2 ounces of water the hot cup was negative, value of deflection ·00005.

The negative condition excited in the hot platinum cup in the solutions of citric and tartaric acid agrees with the results obtained with copper in those liquids.

Fig. 3.



Fig. 4.



I have already shown (*Phil. Mag.* 1857, vol. xiii. p. 1) that the currents obtained with platinum electrodes are not due to the influence of atmospheric air upon the liquid and metal at their line of mutual contact; for, in the experiments there recorded, atmospheric air was entirely excluded, and the liquids were previously well boiled.

To test the influence of *size* of the *cold* electrode, I took a platinum dish, A (see fig. 4), 5 inches wide and  $1\frac{1}{2}$  inch deep, in a glass vessel of the annexed form, B, closed at its lower end by a cork, and containing in its neck two platinum electrodes, one consisting of a wire, C, and the other of a sheet 2 inches long and 2 inches wide in the form of a cylinder, D.

With a cold mixture composed of  $3\frac{1}{2}$  ounces of water and  $\frac{1}{4}$  of an ounce by measure of strong sulphuric acid, and the *sheet* of platinum as the lower electrode, on pouring boiling water into the dish a deflection of the value of .0064 was obtained, the cold electrode being positive; but with the *wire* as the lower electrode no perceptible deflection occurred. These results were obtained repeatedly. The electric currents are therefore largely dependent upon the size of the cold electrode.

#### General Results.

The chief fact brought out conspicuously by these experiments with copper dishes is, that in many cases an increase of chemical action produced by heat, instead of making the hot metal electro-positive, makes it considerably negative.

The results show that hot copper was positive to cold copper in the following liquids:—hydrochloric, hydrocyanic, boracic, and tribasic or orthophosphoric acids; chloride of copper (weak solution); chloride of cobalt; chloride of manganese; chromic acid; chloride of chromium; sulphate of zinc (weak solution); sulphate of magnesia; chloride of calcium; nitrate and chloride of strontium; chloride of barium; nitrate of sodium (strong solution); chloride, iodide, carbonate, and biborate of sodium; sulphate of sodium (strong solution); tribasic phosphate of sodium; nitrate, chloride, and chlorate

of potassium; bromide of potassium (strong solution); iodide of potassium (strong solution); carbonate, acid carbonate, and bichromate of potassium; aqueous ammonia; chloride of ammonium; cyanide and ferrocyanide of potassium; acetate of zinc; and acetate of sodium. And negative in the following ones:—nitric, chloric, hydrobromic, hydrofluosilicic, and sulphuric acids; ferrous sulphate; chloride of copper (strong solution); sulphate of copper; sulphate of zinc (strong solution); nitrate and iodide of sodium (weak solutions); bromide and iodide of potassium (weak solutions); iodate of potassium; chrome alum; nitrate of ammonium; oxalic, acetic, tartaric, and citric acids. The number of liquids in which hot copper was positive was thirty-six, and of those in which it was negative was twenty.

In several instances where the hot metal was negative with a weak solution, it became positive with a strong one—for instance, with sulphate of zinc, nitrate, iodide, and sulphate of sodium, bromide and iodide of potassium; but with chloride of copper the reverse occurred. These results may be connected with the fact that in weak neutral solutions the chemical action is generally the most feeble, and therefore interferes the least with the direct influence of the heat in producing electric currents.

The influence of free hydrochloric, hydrocyanic, boracic, orthophosphoric, and chromic acids was to make the hot copper positive; whilst that of nitric, chloric, hydrobromic, hydrofluosilicic, sulphuric, and some of the organic acids was to make it negative.

In consequence, probably, of the small amount of interference by chemical action in solutions of oxalic, acetic, tartaric, and citric acids, the direct influence of the heat made the copper negative—similar to its influence on platinum in all acid liquids which do not attack that metal.

The nature of the acid in a salt appears to exert much more influence than that of the base on the direction of the current; for instance, in nearly all chlorides, including those of a considerable variety of bases, hot copper was positive, probably because copper is more readily attacked by acids than by bases.

In all decidedly alkaline liquids the hot copper was positive; this is similar to the behaviour of platinum in such solutions, and is probably due to the same cause, viz. the direct influence of the heat, as well as to chemical action.

The results also show that the quantity of the current obtained with any given liquid generally increases with the number of molecules of the substance contained in the solution; in some cases, however, as with sulphuric acid, carbonate of potassium, chloride of ammonium, and acetate of zinc, there was a limit to this increase; and beyond that limit the quantity of the current decreased up to the point of saturation of the liquid.

In the great majority of cases the value of the deflection increased much more rapidly than the strength of the solution, particularly with solutions of sulphate of magnesia, and also of hydrochloric acid and of chloride of sodium, probably because two causes operated,



viz. increased strength of solution and diminished conduction-resistance; in a very few cases, however, the opposite result took place, as with solutions of chloride and nitrate of strontium.

Inversions of the direction of the deflection by difference of strength of the liquid occurred with solutions of chloride of copper, sulphate of zinc, nitrate, iodide, and sulphate of sodium, bromide and iodide of potassium.

Irregularities of the amount of deflection were very apt to take place with liquids which gave strong deflections, or which acted much upon the copper plates (for instance, nitric acid), especially if bubbles of air remained under the plates, or the dishes were wetted on their side above the liquid by the solution.

In certain acid liquids, viz. nitric, chloric, hydrobromic, hydrofluosilicic, and sulphuric acids, the hot copper was strongly negative (notwithstanding the chemical action upon it was distinct, and in some cases even strong); this is similar to the electrical behaviour of platinum in such liquids, and may be attributed either to the more direct influence of the heat alone (such as occurs with platinum plates), or to a different influence of the chemical action produced by the heat. Both these causes probably operate in such cases.

It is probable that in all cases where the hot copper was positive in liquids of strongly acid reaction, the positive condition was due to chemical action alone.

With some liquids, especially with solutions of hydrocyanic, boric, acetic, tartaric, and citric acids, the deflections were very feeble, and the chemical action on the plates not perceptible; whilst with others, such as nitric and chloric acids, solutions of the chlorides of strontium, sodium, potassium, and ammonium, and of carbonate, acid carbonate, and cyanide of potassium, the deflections were considerable, and the chemical action distinct, and in some cases strong. In none of the liquids (except hydrobromic and chromic acids) did the hot plate appear to be *less* stained or corroded than the cold one; probably in all cases it was the most corroded, although in some cases the corrosion was not perceptible.

The amount of deflection was not always proportionate to the amount of chemical action; for instance, with solutions of chloride of copper and iodate of potassium there was considerable corrosion, but only feeble currents, probably because the plates became covered with a badly conducting film, whilst with hydrochloric acid, chloride of cobalt, chloride of manganese, and nitrate of potassium the reverse occurred.

I consider the currents in all these experiments of difference of temperature to be due either, 1st, to the direct influence of heat, the effect of which is to make the hot copper negative in acid liquids and positive in alkaline ones (see *Phil. Mag.* 1857, vol. xiii. p. 1); 2nd, to chemical action, which sometimes overpowers the direct influence of heat and reverses the effect; or, 3rd, to both these influences combined. The more ultimate cause, however, of the phenomena in these cases must be sought for in the *molecular movements* produced by heat in the metals and liquids.

The currents obtained with copper plates were no doubt influenced in their amounts (if not also in their direction) by the oxidizing action of the air upon the liquid and metal at their line of mutual contact; for we know that metals in contact with liquids oxidize much more quickly if oxygen has access to their wet surfaces. And the currents were also influenced by the action of unequal temperature upon this air-contact line; for we know that wet metals oxidize still more rapidly if heat is applied.

*Influence of line of contact of liquid and metal with the air.*

That the length of line of contact of the liquid and copper with the air is capable of producing electric currents was shown by the following experiments:—

Two strips of sheet copper of the annexed form (fig. 5),  $\frac{3}{4}$  inch wide,

Fig. 5.



Fig. 6.



and 12 inches long in the longest limb, were cut from contiguous parts of a sheet of copper, and, after being perfectly cleaned, were coiled into the shape represented by the annexed sketch, fig. 6. They were then placed in a flat-bottomed porcelain dish and connected with the galvanometer, one of the spirals being supported at about  $\frac{1}{4}$  inch higher than the other by means of a triangle of glass rod. The liquid to be examined was then poured into the dish until it just (and completely) covered the lower spiral, and the direction and amount of the permanent deflection noted. The positions of the spirals were then reversed and the electrical effects again noted.

*Experiment 1.*—With a liquid composed of 100 grains of cyanide of potassium dissolved in 12 ounces of water, whichever of the spirals was only partly submerged and therefore had the longest air-line, was strongly electro-negative to the wholly submerged one.

*Experiment 2.*—With a mixture of one measure of strong nitric acid and ten measures of water, deflections of somewhat less amount, but in precisely similar directions to those of experiment 1, took place.

*Experiment 3.*—With dilute hydrobromic acid the directions of the deflections were also similar, but still less in amount.

*Experiment 4.*—With a half-saturated solution of borax very feeble deflections, agreeing in direction with those of the other experiments, were obtained.

These results show the necessity (which I have already mentioned) of excluding air-bubbles from beneath the copper dishes, and of not

wetting the sides of the dishes by the liquid above the level of their immersion.

To ascertain the influence of *difference of temperature* of the air-contact line I soldered two strips of perfectly similar sheet copper, each 12 inches long and  $\frac{1}{2}$  inch wide, in the form of circular hoops 4 inches in diameter upon the bottoms of two tin cups, and ground the edges of the strips perfectly level, and soldered copper wires to them for connecting with the galvanometer. Two glass triangles were now put into the apparatus, fig. 1, one in each dish, to support the cups, and a mixture of one measure of nitric acid and 12 measures of distilled water poured in until it just touched the edges all round of the perfectly horizontal copper rims resting on the triangles. After the needles of the galvanometer had settled at zero, about ten ounces of boiling water was poured into one of the cups; a temporary deflection of the value .0560, and a permanent one of value .0759, were produced, the hot metal being negative. The direction of the current in this experiment agrees with that obtained with the same mixture and the copper dishes; and the result indicates that a large proportion of the quantity of the current obtained with copper dishes in dilute nitric acid was due to the action of the air-contact line.

The influence of the air-line is largely *chemical*. "A piece of copper wire *wholly submerged* in the acid [dilute sulphuric] so as to entirely exclude any portion of it coming into contact with the air, has remained for many months without imparting the slightest tinge to the liquid." "But on suffering the liquid to evaporate so as to bring the upper end of the metal near to its surface, the instant the slightest portion becomes exposed chemical action immediately begins."

"Two equal portions of wire were similarly placed in acid, only that one was fully exposed to the atmosphere in an open tube, while the other was placed in a phial, the acid occupying half its height, and was kept closely corked for several weeks—after which the fully exposed metal had lost in weight two-fifths more than the one which had been excluded from contact with fresh portions of air, showing that contact with the atmosphere in bulk is necessary to the fullest action"\*.

#### *Experiments with Liquids of unequal strength.*

To throw some light upon the questions—1st, Is the quantity of the current simply a result of the difference of number of molecules of liquid which touch the hot plate compared with those which touch the cold plate? and, 2nd, What amount of difference of strength of a liquid is equal to the amount of difference of temperature employed?—I brought the two copper dishes into contact with liquids of unequal strength instead of unequal temperature.

The tube C (fig. 1) was filled with the stronger mixture and closed at its end in the dish A by an india-rubber bung, and the dish B filled

\* "On the Theory of the Voltaic Pile," Bridgman, Phil. Mag. Nov. 1869.



to the line F with the same mixture; the dish A was then filled with the weaker mixture up to the same level and the bung slowly withdrawn. The two copper dishes, previously connected with the galvanometer, were next simultaneously immersed in the mixtures and the effect noted. The following are the results obtained by this method:—

#### *Nitric Acid.*

*Experiment 1.*—In A, 1 volume of acid diluted to 80 volumes. In B, 1 volume diluted to 40. Copper in A was positive temporarily, value  $\cdot 0270$ ; and permanently, value  $\cdot 0198$ .

*Experiment 2.*—In A, 1 volume of acid diluted to 40 volumes. In B, 1 volume of acid diluted to 20 volumes. The copper plate in A was first positive temporarily, value of deflection  $\cdot 0064$ ; and then that in B permanently, value  $\cdot 2850$ .

*Experiment 3.*—In A, 1 volume of acid diluted to 40 volumes. In B, 1 volume diluted to 10 volumes. Copper plate in B was positive temporarily, value  $\cdot 4863$ ; and permanently, value  $\cdot 0819$ .

#### *Hydrochloric Acid.*

*Experiment 1.*—In A, 1 volume of acid diluted to 40 volumes. In B, 1 volume diluted to 20 volumes. The copper in B was positive temporarily, value  $\cdot 9608$ ; and permanently, value  $\cdot 1087$ .

*Experiment 2.*—In A, 1 volume of acid diluted to 40 volumes. In B, 1 volume diluted to 26.66 volumes. The copper in B was positive temporarily, value  $\cdot 3479$ ; and permanently, value  $\cdot 0702$ .

#### *Chloric Acid.*

In A, 1 volume of acid diluted to 80 volumes. In B, 1 volume diluted to 40 volumes. The copper in B was positive temporarily, value  $\cdot 0036$ ; and permanently, value  $\cdot 0009$ .

#### *Sulphuric Acid.*

In A, 1 volume of acid diluted to 80 volumes. In B, 1 volume diluted to 40 volumes. The copper in B was positive temporarily, value  $\cdot 0467$ ; and permanently, value  $\cdot 0330$ .

On examining these results, it will be perceived, 1st, that only in one half the number of the experiments did increased strength of liquid produce electrical currents similar in direction to those produced by increased temperature; and therefore the heat does not act simply by causing a greater number of molecules of each individual substance to touch the hot plate; and, 2nd, that only in one of the experiments was the copper in the weaker liquid both temporarily and permanently positive to that in the stronger; whilst in five of the experiments the copper in the stronger liquid was temporarily and permanently positive to that in the weaker. Increase of strength of the liquid therefore made the copper *positive* in five cases out of six.

In the fourth experiment with hydrochloric acid with difference of temperature, and in the second one with difference of strength, the mixture in each case consisting of 1 volume of acid diluted to 40 volumes with water, an increase of temperature from  $16^{\circ}$  to about  $98^{\circ}$  C. produced a deflection of the value  $\cdot 2854$ , whilst an increase of strength to 1 volume in 26.66 gave a deflection in the same direction of the value  $\cdot 3479$ . An increase of temperature of about  $82^{\circ}$  C. was not quite equal in electrical effect to an increase of 50 per cent. in the number of molecules of the acid which touched the plates.

In the third experiment with chloric acid with difference of temperature, and in the single one made with difference of strength, the mixture in each instance consisting of 1 volume of the acid diluted to 80 volumes with water, an increase of temperature of about  $82^{\circ}$  C. produced an electrical effect of  $\cdot 0040$ ; whilst an increase of 100 per cent. in the number of molecules of the acid which touched the plates produced an opposite electrical effect of  $\cdot 0036$ .

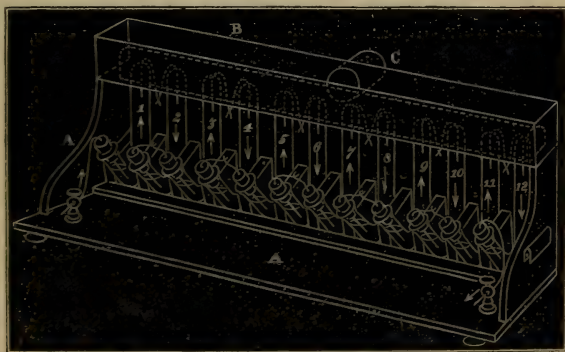
In the third experiment with sulphuric acid with difference of temperature, and in the single one made with difference of strength, each being with a mixture of 1 measure of acid in 80 of water, an increase of temperature of about  $82^{\circ}$  C. caused an electrical effect of  $\cdot 0418$ , and an increase of 100 per cent. in the number of molecules of acid which touched the plates caused an opposite electrical effect of  $\cdot 0467$ .

#### *A liquid thermo-electric battery.*

Acting upon the general results thus obtained in this subject, I constructed a liquid thermo-electric battery consisting of twelve glass tubes,  $\frac{3}{4}$  of an inch in diameter and 10 inches long, closed at one end (and containing a platinum wire hermetically sealed in that

Fig. 7.

Fig. 8.



end), and bent to the form shown in fig. 7, each tube being filled with a conducting liquid, and its outer end closed by a cork, in which was fixed a second platinum wire to dip into the liquid.

Fig. 8 represents the apparatus; A A is a wooden stand supporting

a tin box, B. The box is water-tight, and has in its lower surface a long semicircular cavity (shown by dotted lines) to receive the upper ends of the twelve tubes. To the back of the box is fixed a short cylinder of tin, C, closed at its outer end. When the apparatus is in action, the box is filled with hot water, and the water kept boiling by means of a lamp placed beneath the tube C. The twelve tubes were kept in position by divisions of wood fixed to the back of the stand, as shown in the figure.

The tubes 1, 3, 5, 7, 9, and 11 were filled with a previously boiled and cooled mixture of  $\frac{1}{4}$  of an ounce of sulphuric acid, and 19 ounces of distilled water; and the others, viz. 2, 4, 6, 8, 10, and 12, with a similarly prepared solution of 110 grains of hydrate of potassium dissolved in 19 ounces of distilled water.

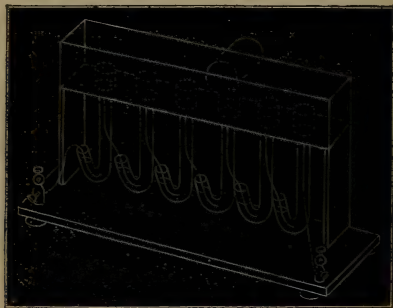
The platinum wires were connected, in the order shown in the sketch, by means of small binding-screws not represented in the figure.

On connecting the terminals with a galvanometer containing about 180 turns of moderately coarse copper wire, and applying heat to the upper electrodes and ends of tubes by means of the boiling water, no deflection of the needles took place; but on substituting a Thomson's reflecting galvanometer, which offered a resistance of 3040·7 B.A. units ( $=77872\cdot327$  miles of copper wire  $\frac{1}{16}$  of an inch thick), a deflection of 40 degrees was readily obtained, the hot platinum wire in the dilute acid being negative, and that in the alkali positive, as shown by the direction of the arrows in the sketch.

From these results it is evident the quantity of the electric current produced was exceedingly small, and its intensity considerable. By employing electrodes of larger surface, such as spirals of platinum wire and more concentrated liquids, the quantity of the current would be very largely increased. (See *Phil. Mag.* 1857, vol. xiii. p. 1.)

Fig. 9 represents a simpler arrangement of this apparatus, in which only one kind of liquid, either acid or alkaline, is employed. The

Fig. 9.



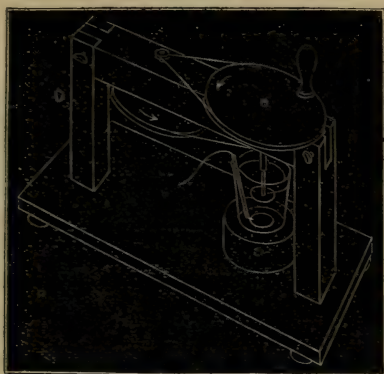
electrodes in this arrangement must be disposed in the order represented by the figure.



*Influence of Friction.*

To ascertain if the *friction* of one of the electrodes against the liquid had similar effects to those produced by the direct application of heat, I employed the apparatus shown in fig. 10. The sketch does not require explanation.

Fig. 10.



*Experiment 1.*—By immersing two stout copper wires vertically in an acidulated solution of cupric sulphate and rotating one of them at a speed of about 5000 revolutions per minute, the rotating wire became electropositive.

*Experiment 2.*—With a saturated solution of borax, the rotating wire was positive.

*Experiment 3.*—With a solution of cyanide of potassium, the rotating wire was negative.

*Experiment 4.*—With stout platinum wires in an acidulated solution of cupric sulphate, the rotating wire became negative.

*Experiment 5.*—With platinum wires in a solution composed of 200 grains of carbonate of potassium in 40 ounces of distilled water, the rotating wire was faintly positive, and similarly in a very dilute solution.

*Experiment 6.*—With two platinum disks one above the other in a strong solution of carbonate of potassium, revolving the upper disk at a speed of about 5000 revolutions per minute made it electropositive.

*Experiment 7.*—With an acidulated solution of cupric sulphate, the revolving disk became feebly negative.

On comparing these results with those obtained by unequal temperature, we find that the directions of the currents in the two classes of cases were reverse with copper in solutions of acidulated cupric sulphate and cyanide of potassium, and similar in a solution of borax ; and with platinum in solutions of acidulated cupric sulphate or carbonate of potassium the influence of friction and of increased tem-

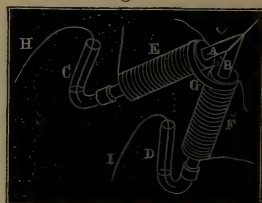
perature upon the direction of the currents were the same. The molecular movements, therefore, produced by friction are not in all cases similar to those produced by heat.

*Influence of Magne-optic rotating-power of the Liquids.*

Being desirous of determining whether the thermo-electric properties of liquids were dependent on the molecular structure by virtue of which liquids under the influence of magnetism polarize light circularly, I made the following apparatus and experiment:—

A and B (fig. 11) are two straight glass tubes, about  $\frac{3}{4}$  inch in diameter and 10 inches long, with two

Fig. 11.



similar (but bent) tubes, C and D, attached to their free ends by india-rubber tubing. The sloping ends of the straight tubes are ground flat, and are joined together securely at their edges by melted shellac, with a thin and projecting sheet of platinum between them to separate the liquids. E and F are two strong electro-helices wound upon stout tubes of soft iron which enclose the glass tubes. The apparatus is secured upon a board in an inclined position with the sloping ends of the tubes uppermost; and the two helices are held together at their upper ends by an india-rubber band, G. I filled one of the tubes with a clear and strong solution of perchloride of iron (of negative magne-optic rotatory power, see Verdet, *Phil. Mag.*, June 1858), and the other with a similar solution of chloride of nickel (of positive magne-optic rotatory power), and connected the liquids in the bent tubes with a galvanometer 16 feet distant by means of the platinum wires H and I. I now excited the helices in various ways by means of 12 strong Grove's cells; no current was induced in the liquid. I next heated the junction of the tubes gradually; the solution of iron became thermo-electro-positive, and a steady but feeble deflection of the needles took place; and during the continuance of this current I again excited the helices in various ways as before; again no electrical effects were produced. The results of this experiment strongly support the conclusion that the thermo-electric properties of liquids are not dependent upon the magne-optic polarizing power of the liquids, nor upon the properties of their *mass*.

On examining the thermo-electric properties of the solution of ferric chloride with platinum plates in the apparatus described in the '*Philosophical Magazine*,' 1857, vol. xiii. p. 1, the hot platinum was strongly negative, value of temporary deflection '8475. With the nickel solution, similarly examined, the hot plate was also negative, value of deflection '0409. These results agree with that obtained with the two tubes in the last experiment, the more positive condition of the iron solution than that of the nickel one determining the direction of the current in that experiment.

*General Conclusion.*—The electric currents produced by the direct

influence of unequal temperature or friction of platinum or copper electrodes, in conducting liquids which do not act chemically upon those metals, have their origin in temporary changes of cohesion of the layers of metal and liquid which are in immediate and mutual contact, and may be considered a very delicate test of the kind and amount of temporary molecular movements produced by those causes.

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GEOLOGICAL SOCIETY.

[Continued from vol. xlii. p. 389.]

June 7, 1871.—Joseph Prestwich, Esq., F.R.S., President,  
in the Chair.

The following communications were read :—

1. "On the persistence of *Caryophyllia cylindracea*, Reuss, a Cretaceous Coral, in the Coral-fauna of the Deep Sea." By P. Martin Duncan, M.B. Lond., F.R.S., F.G.S., Professor of Geology in King's College, London.

The author first referred to the synonyms and geological distribution of *Caryophyllia cylindracea*, Reuss, which has hitherto been regarded as peculiar to the White Chalk, and as necessarily an extinct form, inasmuch as it belonged to a group possessing only four cycles of septa in six systems, one of the systems being generally incomplete. The distribution of the *Caryophylliæ* of this group in the Gault and the Upper Chalk, the Miocene, and the Pliocene was noticed, and also that of the species with the incomplete cycle. The falsity of this generalization was shown to be proved by the results of deep-sea dredging off Havannah, under Count Pourtales, and off the Iberian peninsula under Dr. Carpenter and Mr. Gwyn Jeffreys. The former dredged up *Caryophyllia formosa* with four complete cycles; and the latter obtained, from depths between 690 and 1090 fathoms, a group of forms with four complete and incomplete cycles. This group had a Cretaceous facies; one of the forms could not be differentiated from *Caryophyllia cylindracea*, Reuss; and as a species of the genus *Bathycyathus* was found at the same time, this facies was rendered more striking. The representation of the extinct genera *Trochosmilia*, *Parasmilia*, *Synhelia*, and *Diblasus* by the recent *Amphiheliæ*, *Paracyathi*, and *Caryophylliæ* was noticed; and it was considered that, as the Cretaceous forms thrived under the same external conditions, some of them only being persistent, there must be some law which determines the life-duration of species like that which restricts the years of the individual. It was shown that deep-sea conditions must have prevailed within the limits of the diffusion of the ova of coral polyps somewhere on the Atlantic area ever since the Cretaceous period.

2. "Note on an *Ichthyosaurus* (*I. enthekiodon*) from Kimmeridge Bay, Dorset." By J. W. Hulke, Esq., F.R.S., F.G.S.

In this paper the author described the skeleton of an *Ichthyo-*



*saurus* from Kimmeridge Bay, agreeing in the characters of the teeth with the form for which he formerly proposed the establishment of the genus *Enthekiodon*. The specimen includes the skull, a large portion of the vertebral column, numerous ribs, the bones of the breast-girdle, and some limb-bones. The first forty-five vertebral centra have a double costal tubercle. The coracoids have an unusual form, being more elongated in the axial than in the transverse direction, and this elongation is chiefly in advance of the glenoid cavity. The articular end of the scapula is very broad. The paddles are excessively reduced in size, the anterior being larger than the posterior, as evidenced by the comparative size of the proximal bones. The species, which the author proposed to name *I. enthekiodon*, most nearly resembles the Liassic *I. longirostris* and *I. tenuirostris*. The length of the preserved portion of the skeleton is about 10 feet; the femur measures only 2 inches, and the humerus 2·7 inches.

3. "Note on a Fragment of a Teleosaurian Snout from Kimmeridge Bay, Dorset." By J. W. Hulke, Esq., F.R.S., F.G.S.

In this paper the author described a fragment of the snout of a Teleosaurian obtained by J. C. Mansel, Esq., F.G.S., from Kimmeridge Bay, and which is believed to furnish the first indication of the occurrence of Teleosaurians at Kimmeridge. The specimen consists of about 17 inches of a long and slender snout, tapering slightly towards the apex, where the præmaxillæ expand suddenly and widely. The nostril is terminal and directed obliquely forwards; the præmaxillæ ascend 2·5 in. above the nostril, and terminate in an acute point; and each præmaxilla contains five alveoli. The lateral margins of the snout are slightly crenated by the alveoli of the teeth, of which the three front ones are smaller than the rest: most of the teeth have fallen out; but a few are broken off, leaving the base in the sockets.

## IX. Intelligence and Miscellaneous Articles.

AN EXPLOSION ON THE SUN. BY C. A. YOUNG.

ON the 7th of September, between half-past 12 and 2 P.M., there occurred an outburst of solar energy remarkable for its suddenness and violence. Just at noon the writer had been examining with the telespectroscope\* an enormous protuberance of hydrogen cloud on the eastern limb of the sun.

It had remained with very little change since the preceding noon—a long, low, quiet-looking cloud, not very dense or brilliant, nor in any way remarkable except for its size. It was made up mostly of filaments nearly horizontal, and floated above the chromosphere†

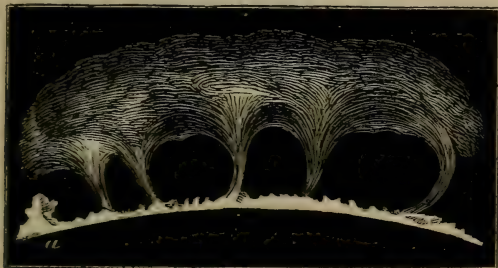
\* This is the name given by Schellen to the combination of astronomical telescope and spectroscope.

† The *chromosphere* (called also *sierra* by Proctor and others) is the

with its lower surface at a height of some 15,000 miles, but was connected to it, as is usually the case, by three or four vertical columns brighter and more active than the rest. Lockyer compares such masses to a banyan grove. In length it measured  $3' 45''$ , and in elevation about  $2'$  to its upper surface; that is, since at the sun's distance  $1''$  equals 450 miles nearly, it was about 100,000 miles long by 54,000 high.

At  $12^h 30^m$ , when I was called away for a few minutes, there was no indication of what was about to happen, except that one of the connecting stems at the southern extremity of the cloud had grown considerably brighter, and was curiously bent to one side; and near the base of another, at the northern end, a little brilliant lump had developed itself, shaped much like a summer thunder-head. Fig. 1 represents the prominence at this time, *a* being the little "thunder-head"\*.

Fig. 1.



What was my surprise, then, on returning in less than half an hour (at  $12^h 55^m$ ), to find that in the mean time the whole thing had been literally blown to shreds by some inconceivable uprush from beneath. In place of the quiet cloud I had left, the air, if I may use the expression, was filled with flying *débris*—a mass of detached vertical fusiform filaments, each from  $10''$  to  $30''$  long by  $2''$  or  $3''$  wide, brighter and closer together where the pillars had formerly stood, and rapidly ascending.

When I first looked, some of them had already reached a height of nearly  $4'$  (100,000 miles); and while I watched them they rose with a motion almost perceptible to the eye, until in ten minutes ( $1^h 05^m$ ) the uppermost were more than 200,000 miles above the solar surface. This was ascertained by careful measurement; the mean of three closely accordant determinations gave  $7' 49''$  as the extreme altitude attained; and I am particular in the statement, because, so far as I know, chromospheric matter (*red hydrogen* in this case) has never before been observed at an altitude exceeding  $5'$ .

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layer of hydrogen and other gasses which surrounds the sun to a depth of about 7000 miles. Of this the prominences are mere extensions.

\* The sketches do not pretend to accuracy of detail, except the fourth; the three *rolls* in that are nearly exact.

The velocity of ascent also, 166 miles per second, is considerably greater than any thing hitherto recorded. A general idea of its appearance, when the filaments attained their greatest elevation, may be obtained from fig. 2.

As the filaments rose they gradually faded away like a dissolving cloud; and at 1<sup>h</sup> 15<sup>m</sup> only a few filmy wisps, with some brighter streamers low down near the chromosphere, remained to mark the place.

But in the meanwhile the little "thunder-head," before alluded to, had grown and developed wonderfully into a mass of rolling and ever-changing flame, to speak according to appearances. First it was crowded down, as it were, along the solar surface; later it rose almost pyramidally 50,000 miles in height; then its summit was drawn out into long filaments and threads which were most curiously rolled backward and downward, like the volutes of an Ionic capital; and finally it faded away, and by 2<sup>h</sup> 30<sup>m</sup> had vanished like the other.

Figs. 3 and 4 show it in its full development—the former having been sketched at 1<sup>h</sup> 40<sup>m</sup>, and the latter at 1<sup>h</sup> 55<sup>m</sup>.

Fig 2.



Fig. 3.

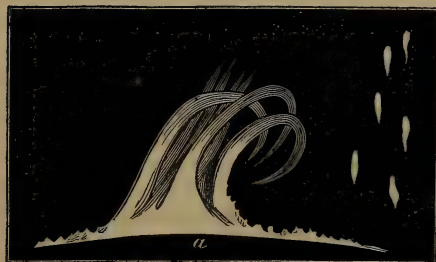
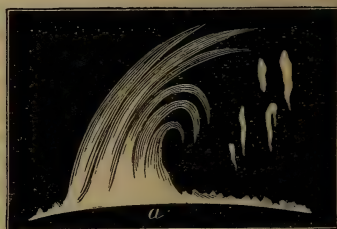


Fig. 4.



The whole phenomenon suggested most forcibly the idea of an *explosion* under the great prominence, acting mainly upward, but also in all directions outward, and then after an interval followed by



a corresponding in-rush : and it seems far from impossible that the mysterious coronal streamers, if they turn out to be truly solar, as now seems likely, may have their origin and find their explanation in such events.

The same afternoon a portion of the chromosphere on the opposite (western) limb of the sun was for several hours in a state of unusual brilliance and excitement, and showed in the spectrum more than 120 bright lines, whose position was determined and catalogued, —all that I had ever seen before, and some 15 or 20 besides.

Whether the fine aurora borealis which succeeded in the evening was really the earth's response to this magnificent outburst of the sun is perhaps uncertain ; but the coincidence is at least suggestive, and may easily become something more if, as I somewhat confidently expect to learn, the Greenwich magnetic record indicates a disturbance precisely simultaneous with the solar explosion.—Silliman's *American Journal*, Dec. 1871.

Dartmouth College, U.S., September 1871.

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#### ON THE TRANSVERSE VIBRATIONS OF WIRES AND THIN PLATES.

BY M. E. GRIPON.

Wires and thin plates of metal, fixed to the branch of a diapason and having one extremity free, vibrate, by communication, after the manner of rods and in unison with the diapason. The same as in Melde's experiments on stretched wires, on these little rods a certain number of loops are distinguished separated by nodes. The equations of the motion of the wire, regarded either as a string or as a rod, can be found by analysis. The results of the calculation are completely verified by experiment.

The number of the nodes varies with the length of the rod ; the distances from each of these nodes to the free extremity are independent of its length. The relative distances of the nodes are exactly the same as on a rod fixed at one extremity and free at the other, with one exception—that the extremity fixed to the diapason is not always a node, but its distance from the nearest node varies with the length. In general the rod does not vibrate, or vibrates irregularly, when the node ought, theoretically, to be formed at the point of attachment of the rod to the diapason.

The normal distances of two consecutive nodes are inversely proportional to the square roots of the numbers of vibrations of the diapasons employed, and *cæteris paribus* proportional to the square roots of the thicknesses of the rods. The accord of theory with experiment is so far complete that the velocity of sound in the rod used can be determined by measuring the normal distance between two consecutive nodes, the thickness of the rod, and ascertaining the number of the vibrations of the diapason.

This very simple method leads, for the usual metals, to the numbers already found by Wertheim and Masson. It is perfectly applicable to the case in which we have to do with very flexible substances, such as paper ; in these cases the usual methods are defective.

By such experiments we can ascertain the variations which humidity induces in the elasticity of hygrometric substances, such as paper.

The laws of vibrating strings are verified on very fine wires, except when the tension of the wire is not more than a few grammes.

In all these experiments, whether on strings or rods, the position of the nodes is not rigorously fixed; the nodes, especially those near the diapason, shift during the motion; their distance from the diapason increases in proportion as the amplitude of the vibrations grows less.

Free rods, or wires but slightly stretched, present another anomaly. When the vibrations have very little amplitude, the wires take the mode of division indicated by theory, and the point of attachment is not a node: but if the diapason is made to vibrate more and more forcibly, the number of the nodes decreases; thus instead of four nodes, only three, two, or one is observed, or even none at all. The point of attachment of the wire to the diapason is then the place of a node, and the wire is divided by the nodes into equal parts. Free rods, therefore, exhibit, according to the stroke, a variable number of nodes, the arrangement of which follows the ordinary laws.

The wire, in all cases, continues to vibrate in unison with the diapason; or at least no sound foreign to that of the instrument is heard. Here is an anomaly which the theory does not appear to account for; perhaps it results from the circumstance that, in constructing the differential equation of the motion of the wire, the amplitudes of the vibrations were supposed infinitely small.—*Comptes Rendus de l'Acad. des Sciences*, 1871, Nov. 20, pp. 1213-1215.

ON A NEW PHENOMENON OF PHOSPHORESCENCE PRODUCED BY  
FRICTIONAL ELECTRICITY. BY M. ALVERGNIAT.

We make a vacuum, by means of the mercurial air-pump, in straight tubes of glass 45 centims. long; we then introduce a small quantity of chloride or bromide of silicium, and continue the exhaustion till the pressure is reduced to 12 or 15 millims., after which we close the tube at the lamp.

If the tube thus prepared be then rubbed between the fingers or with a piece of silk, a bright glimmer within the tube is seen to follow the movement of the rubber; it is rose-coloured with the chloride, and greenish yellow with the bromide of silicium. It recalls that which has long been observed in the barometer, but it is brighter.

We will remark that, if we try to cause the spark of the induction-coil to pass in these tubes, it develops no light there, unless the vacuum be more perfect; but then the phosphorescence excited by the friction disappears.

We owe our thanks to M. Friedel for the preparation, in a state of purity, of the substances which have given this result.—*Comptes Rendus de l'Acad. des Sciences*, 1871, Nov. 20, p. 1215.

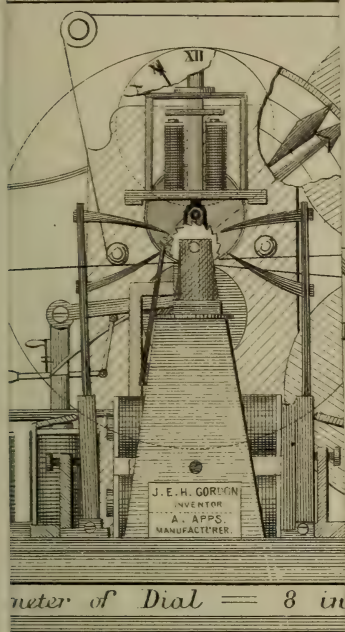


Fig. 3.

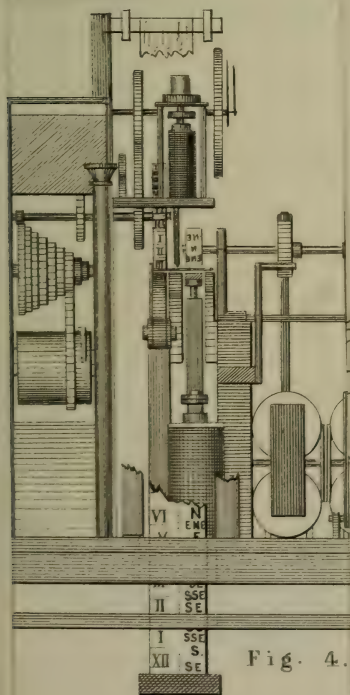
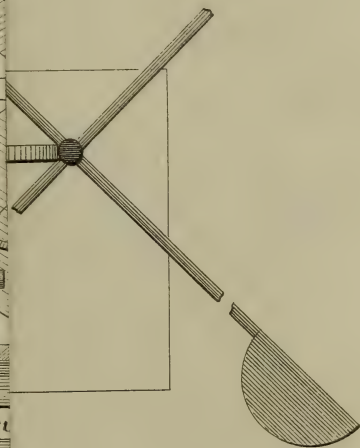


Fig. 4.

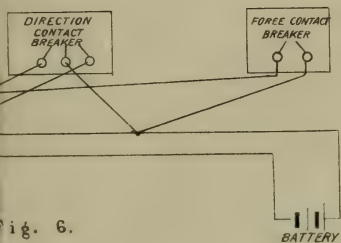
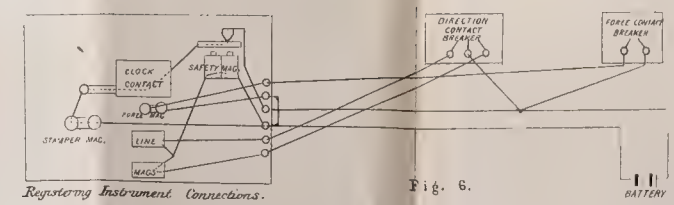
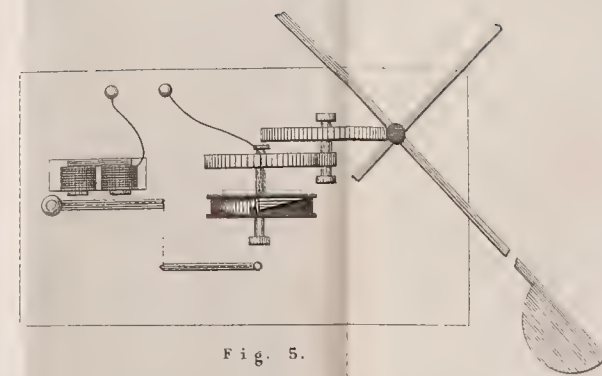
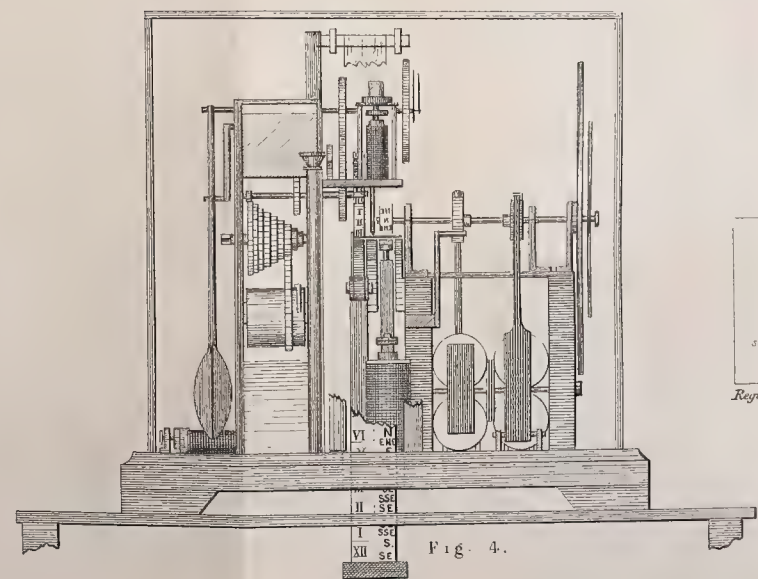
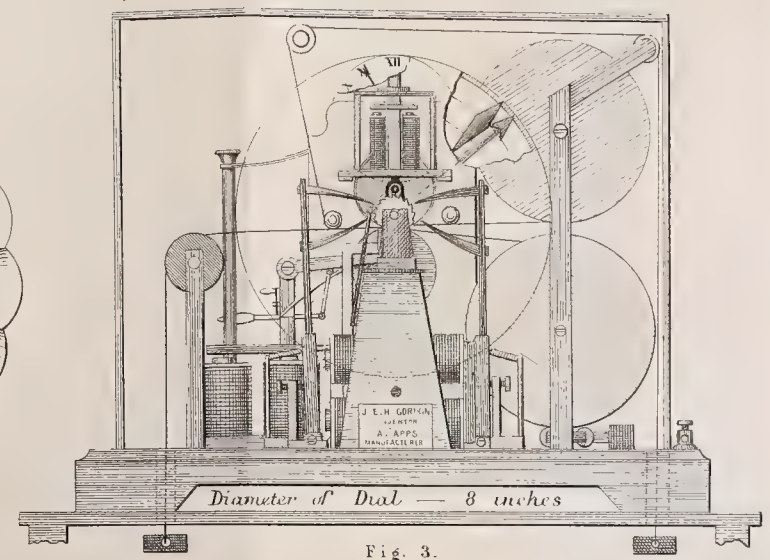
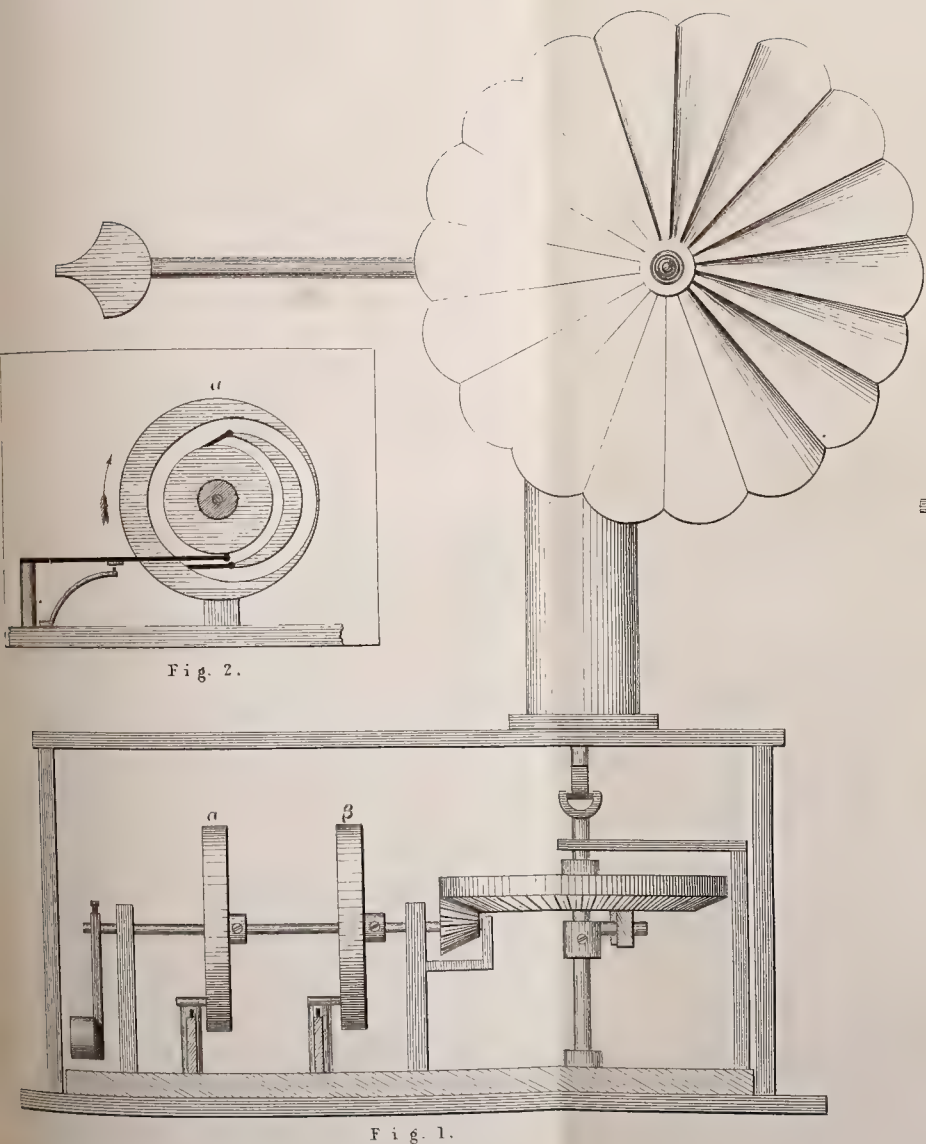


Fig. 6.











THE  
LONDON, EDINBURGH, AND DUBLIN  
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[FOURTH SERIES.]

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FEBRUARY 1872.

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X. *Researches on the Electromotive Force in the Contact of Metals, and on the Modification of that Force by Heat.* By E. EDLUND\*.

§ 1.

TWO essentially different theories have gained acceptance in the explanation of the origin of the galvanic current. Volta himself, the creator of the galvanic pile, admitted that the contact of heterogeneous bodies was sufficient of itself to produce a development of electricity, even if those bodies exercised no chemical action on one another. According to him, chemical affinity between the bodies whose reciprocal contact excited the electric current, was of no importance, or at least singularly subordinate, in an electromotive point of view. Volta based his opinion chiefly on his fundamental experiments, by which he believed he could demonstrate that two chemically indifferent metals produce by their reciprocal contact a decomposition of the electricity—the one becoming electropositive, and the other electronegative. This is the so-called theory of contact, which, without any important modification, has hitherto been admitted as correct by a great number of scientific men.

Nevertheless weighty objections were early made to the theory of contact, by several eminent physicists, who endeavoured to demonstrate that chemical affinity between the bodies whose contact called forth electrical phenomena was of preponderant importance in the development of electricity. Among other

\* Translated from a separate impression, communicated by the Author, from the *Kongl. Svenska Vetenskaps-Akademiens Handlingar*, vol. ix. No. 14.

objections to the validity of Volta's fundamental experiments, the defenders of the electro-chemical theory contended that it was impossible to be certain that the decomposition of the electricity at the time of the contact of the two metals really proceeds from that contact—that, on the contrary, it might have its true cause in the circumstance that the surface of the metal is covered with a layer of moisture or of condensed gas, to which it is impossible to deny a chemical affinity with the metal which they cover, and, consequently, that contact between the metal and the layer of gas or moisture is the real cause of the development of electricity. In order to refute this objection, Volta's fundamental experiments were repeated in various gases and *in vacuo*; and it was ascertained that electricity is quite as well developed as when the experiments are made under the usual conditions. This attempt at refutation called forth the reply that, as experiment has demonstrated in other cases, condensed gases do not entirely disappear *in vacuo*—that consequently the electromotive force admitted to exist between the metals and the gases or moisture at their surface may exist all the same in the vacuum which has been made. It cannot, then, be maintained that Volta's fundamental experiments, as performed with the aid of the electroscope, have furnished an entirely irrefutable proof of the presence of an electromotive force in the contact between metals. It must, then, be confessed that this question has not yet been solved in the way followed for the purpose with the aid of the electroscope\*.

The phenomena, however, discovered by Peltier, of galvanic cooling and heating, have furnished a perfectly distinct reply to the question as to the existence of an electromotive force in the contact between two different metals. Peltier found (as is known) that, if a galvanic current passes through the point of contact established between two different metals, that point becomes heated or cooled, according to the direction of the current.

If the current passes in a certain direction, there results a true production of heat; if in the opposite direction, the result is an absorption of heat. It has been found by experiment that the variations of temperature produced at the point of contact are proportional to the intensity of the current. With the aid of the mechanical theory of heat, and of some known theses derived from the theory of electricity, I have proved, in a previous work†, that Peltier's phenomena are explained with the greatest facility

\* Wiedemann, *Die Lehre vom Galvanismus und Erdmagnetismus*, T. 2, § 849.

† *Öfversigt af Vet. Akademiens Förhandl. för 1869*, p. 457. *Pogg. Ann.* vol. cxxxvii. p. 474. *Arch. des Sciences Phys. et Nat.* vol. xxxvi. p. 214. *Phil. Mag.* S. 4. vol. xxxviii. p. 263.

if we admit the presence of an electromotive force at the point of contact between the two metals. If the galvanic current passes in the same direction as the current produced by the electromotive force, a cooling results at the point of contact, which is changed into a rise of temperature in the opposite case. The theory further demonstrates that the quantities of heat produced or absorbed are proportional to the electromotive forces, and consequently are found to be in full conformity to that which experiment requires\*. In the present state of science it is impossible to account for the modifications of temperature noted at the surface of contact without admitting the presence of an electromotive force at that surface. Experiments have decisively proved that a quantity of heat actually disappears at the surface of contact, and not merely that a less production of heat takes place there than in other parts of the circuit; Lenz's experiments have given evident proof of this fact. Now it is impossible that heat should disappear without producing mechanical work, external or internal, which is then the equivalent of the vanished heat, or without its changing into another form of motion. The mechanical work which ought to arise as the equivalent of the heat which has disappeared could only consist of an increase of disgregation; but there is no sufficient reason for the existence of such a modification; far from that, we possess valid proofs of the contrary, namely that there exists no disgre-

\* M. Clausius, to whom science is much indebted for the most important discoveries in the mechanical theory of heat, has already, before me, treated Peltier's phenomena after the principles of the above-mentioned theory (Pogg. *Ann.* vol. xc. p. 513), and called attention to this circumstance in consequence of my work above indicated (Pogg. *Ann.* vol. cxxxix. p. 280). After having noted that M. Helmholtz's supposition, that all the electrical phenomena which take place in metallic conductors are easily explained if we attribute to the different chemical substances a power of attraction different for the two electricities, is not correct, for this is not sufficient to explain the whole of those phenomena, M. Clausius continues as follows:—"For the explanation of thermoelectric currents, and of the phenomena discovered by Peltier, viz. the heating and cooling occasioned at the point of contact of two substances by an electric current, this assumption does not suffice; for that purpose another assumption is necessary, namely that the heat itself is operative in the production and preservation of the electrical difference at the point of contact, in that the molecular motion which we call heat tends to expel the electricity from one substance to the other, and can only be prevented from doing so by the counteracting force of the two layers of electricity thereby formed, when these have attained a certain density." Now, if we admit the correctness of this hypothesis, Peltier's phenomena can, as M. Clausius has shown, at once be explained by it; if, on the contrary, the validity of it can be called in question, those phenomena remain still unexplained. In order to show the connexion of the phenomena with the electromotive forces at the surface of contact, I have thought it necessary to start from no sort of hypothesis, but solely and exclusively from the known facts and circumstances.



gation or modification of the position of the molecules at the surface of contact. The intensity of the thermoelectric force in the contact of two different metals depends, to a high degree, on the molecular condition of the metals. If that condition undergo a modification (as, for example, by tension, compression, &c.), the result is also a modification in the thermoelectric force. Now we know that, if one of the points of the soldering of a thermoelectric ring be heated, we obtain, for a determinate difference of temperature between the surfaces of union, a current of given amount, which remains constant as long as the respective temperatures of those surfaces remain unchanged. If, now, the metals underwent a molecular modification at the hotter point of contact (where the heat is absorbed), the current could not preserve a constant force, in consequence of that molecular modification; but as this is the case, it follows that no sensible molecular modification can take place there. We must therefore, in order to give an adequate explanation of the disappearance of the heat, admit that it is changed into another form of motion; and of all those with which we are acquainted, electricity is the only one that can here be taken into consideration.

The proof given by me, in the work mentioned, of the presence of an electromotive force in the contact between metals, is not founded on a mere hypothesis, but on facts demonstrated by experiment; the result obtained (namely, the presence of an electromotive force at the surface of contact, as the only possible explanation of Peltier's phenomena) must therefore be correct. This force transforms heat into electricity. It does not create the electric motion out of nothing, but, I repeat, changes heat into electricity. The electric motion produced is the mechanical equivalent of the heat which has vanished. If heat did not exist at the surface of contact, the rise of an electric current would be impossible; for (if I may so express myself) the materials for the production of electricity would be wanting. The force of contact resembles in this respect the inductive force which, when a closed circuit is brought near a galvanic current, transforms into an induced galvanic current the mechanical work necessary to produce the nearness.

It has been maintained\* that the contact of chemically indifferent bodies could only produce a galvanic current of momentary, not of long duration. "It may be admitted," it has been said, "that, in the approach of two metals, the particles are mutually attracted, move rapidly against each other, and at length lose, at their mutual contact, their acquired velocity. The *vis viva* thus lost might be changed into electrical decomposition of

\* Wiedemann, *Die Lehre vom Galvanismus und Erdmagnetismus*, part ii. § 849; cf. Helmholtz, *Erhaltung der Kraft*.

such a nature as to produce, under certain circumstances, a galvanic current. But it is clear that such a current could only have a momentary duration. Immediately after contact the particles would be at rest; the rise of a galvanic current would be impossible, seeing that it cannot be created out of nothing. If, on the contrary, the bodies act chemically on each other, the particles enter, after the contact, into chemical combination, new particles are attracted and lose their acquired velocity, and thus there exist materials for the formation of a galvanic current as long as the chemical activity continues." If the *vis viva* lost by the contact of the particles after their approach were the only element suited to produce a galvanic current, the proof above exhibited would be perfectly valid, and it would be absolutely impossible that contact between bodies chemically indifferent should bring about a galvanic current of a certain duration. But the elements necessary for the formation of the galvanic current are not composed of the *vis viva* lost in the collision of the particles. The explanation of Peltier's experiments demonstrates that it is the heat which is transformed into electricity. The opinion above quoted, therefore, proves nothing with respect to the presence of an electromotive force in the surface of contact between chemically indifferent bodies.

It is proved that, for one and the same force of the current, the quantities of heat lost, as well as those produced, in Peltier's experiments, are proportional to the electromotive forces at the surfaces of contact. By the appreciation of these quantities, then, we gain a relative measure of the electromotive forces in question. I give below the account of the experiments undertaken by me for the purpose of measuring the electromotive forces springing from the contact of metals. The researches I have already published on this subject\* must only be regarded as preliminary. Not only was the method employed not sufficiently delicate for the measurement of the smallest of the forces, but certain arrangements did not permit the obtaining of results with perfect certainty and rigour. I have done my best to get rid of these imperfections in the researches which are described below†.

\* *Öfversigt af Vet. Akademiens Vörhandling. för 1870*, p. 3. *Pogg. Ann.* vol. cxl. p. 435. *Phil. Mag.* S. 4. vol. xli. p. 18.

† On this subject we are indebted to M. le Roux for some very meritorious researches, published in *Annales de Chimie et de Physique*, vol. x.; but his method is, from beginning to end, totally different from that employed by me. To measure Peltier's quantities of heat, M. le Roux used two calorimeters, each containing an equal volume of water, and placed side by side. To ascertain the quantities of heat lost or gained in the contact of two metals—for example, copper and bismuth—one surface of contact was placed in each calorimeter, and the current was conducted so

## § 2.

When a galvanic current passes through a wire composed of two different metals, there result two sources of heat, one independent of the other: in the first place, in consequence of the galvanic resistance, a quantity of heat proportional to that resistance and to the square of the intensity of the current is developed in the wire; in the second place, there ensues at the point of contact a production or absorption of heat proportional both to the electromotive force of that point and to the intensity of the current. The first source of heat is in general, but above all with metallic wires which present great resistance, infinitely greater than the second, and, besides, increases more rapidly with the intensity of the current. If, then, in an experiment the intensity varies a little, it is not impossible that the variations in the quantity of heat produced by the resistance may be greater than the total quantity of heat to be measured, and consequently the result become doubtful. For the accuracy of the measurements, therefore, it is evidently of great importance to give to the apparatus such a construction that its indications shall be perfectly independent of the first source of heat. Whatever the principle on which the apparatus is constructed, and however in other respects it be arranged, it is further necessary that those indications depend as little as possible on foreign influences of temperature (as, for example, variations in the temperature of the place in which the experiments are made, &c.)—a condition difficult to fulfil, seeing that the sensitiveness of the apparatus ought to be so great that it will indicate the slightest variations of temperature. It is therefore necessary to regulate the experiments in such a manner that those influences of foreign temperature can be eliminated, if we cannot render ourselves perfectly independent of them. The question is, to measure the

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as to pass from the copper to the bismuth in one of the calorimeters, and *vice versa* in the other. The water of one of the calorimeters, therefore, became hotter than the water of the other; and the difference between their temperatures, measured by means of an ordinary thermometer, gave the measure of the quantities of heat sought. This method has the advantage of giving, in ordinary units of heat, the measure of the quantities in question. On the other hand, it is not so delicate, by far, as that employed by me. M. le Roux obtained, for the bismuth-copper combination, a difference of temperature of 1.7 degree. With a combination for which the absorption or production of heat is a fiftieth or a hundredth part of the above, the total difference of temperature to be measured by the mercurial thermometer would only amount to some hundredths of a degree; the measurement of such a difference by means of thermometers of which the divisions are tenths of a degree cannot be particularly exact. Doubtless, also, this is in part the cause of the differences between M. le Roux's results and mine.



quantities of heat produced or lost at the surfaces of contact, and not merely the variations of temperature which have been produced there. In order to find the former by the help of the latter, it is necessary to know, under the control of ordinary circumstances, the calorific capacity of the body in which the change of temperature is effected. But always to endeavour to measure the capacity for heat of the metals submitted to experiment would take too long, and would with difficulty lead to perfectly sure results. The experiments will therefore be regulated so that the calorific capacity shall not exercise any sensible influence on the exactness of the results. In order to fulfil these conditions as far as possible, I contrived a kind of air-thermometer, of the following peculiar construction :—

Fig. 1.

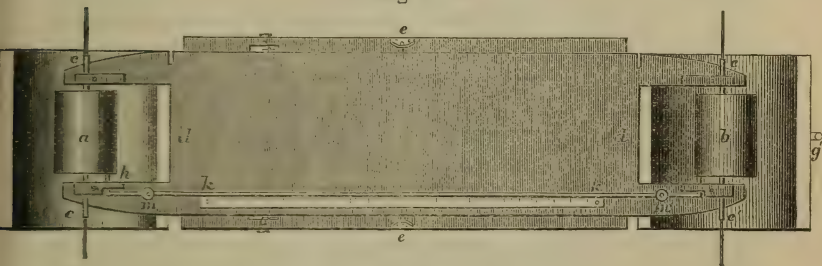


Fig. 2.

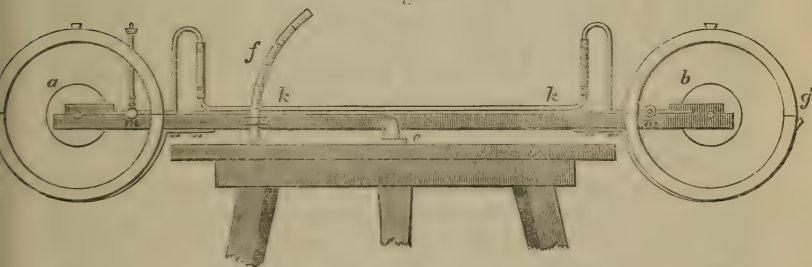


Fig. 1 represents the apparatus viewed from above; and fig. 2 is a side view. *a* and *b* are two perfectly equal cylinders of thin sheet copper, 125 millims. long, and 80 in diameter, with their outer surface silvered. To the centre of their circular ends the brass tubes *c*, *c'* are soldered. Through these tubes, which are placed opposite to one another and make a right angle with the ends of the cylinder, are introduced the wires intended for the experiments, in such a manner that the surface of soldering between the two wires is nearly in the middle of the cylinder. The cylinders are supported by a mahogany board *dd*, which,

for this purpose, is hollowed out at its two extremities into the form of a fork, between the branches of which the cylinders are placed, and on which the tubes  $c, c, c', c'$  rest and can be fixed. The board  $dd$  is moveable on the horizontal axis  $e$ , fixed to its centre, so as to make different angles with the horizontal plane. The inclination of the board to the horizontal is read on the graduated plate  $f$ , by means of which the board can also be fixed in the position desired. In one of the ends of each cylinder, at  $h$ , a brass tube is fixed. These tubes, which form a right angle with the ends, are themselves bent at a right angle, so as to run parallel with the surface of the ends, and then rise in the manner indicated in fig. 2. The brass tubes are put in communication with each other through the glass tube  $k$ , the extremities of which are curved upwards in order to adapt them to the extremities of the brass tubes. The glass tube is joined to the brass ones by caoutchouc tubes, which are tightly bound in order to render the joint impermeable to the air. Into the part of the brass tubes parallel with the ends of the cylinders the brass cocks  $m, m$  are fitted, their plugs being pierced in the form of a T. Each tube has a lateral aperture at right angles with the longitudinal axis of the tube and opposite to an aperture of the cock when one of the others opens the communication with the cylinder or with the glass tube. It clearly follows from this, that, by giving the cock a suitable position, one can either put the glass tube in communication with the copper cylinder and close them both to the external air, close the cylinder and put the glass tube in communication with the air, close the tube and open the cylinder, or at the same time put in reciprocal communication the tube, the cylinder, and the air. The glass tube, having an internal diameter of 2.5 millims., rests on a brass scale which is fixed to the mahogany board and divided into millimetres.

For the purpose of protecting the copper cylinders against the variations of the temperature of the laboratory, they were each entirely covered with a jacket ( $g, g$ ) of sheet zinc. These jackets had double walls, and would each contain 5.8 kilogrammes of water. Their internal diameter was 140 millims.; so that, the copper cylinder measuring 80, the annular space filled with air which separated them had a thickness of 30 millims. In order that the jackets might be easily taken off and replaced, they could be separated into four pieces accurately fitting one another. In fig. 1 the upper halves as well as the ends of the jackets, and in fig. 2 the ends alone, are removed. The two jackets were exactly equal in dimensions and alike in shape; and their outer surfaces, as well as those toward the copper cylinders, had been polished, and then varnished to pre-

vent them from oxidizing so readily. They had also apertures for the passage of the before-mentioned brass tubes, of the brass collars which served for turning the cocks, and of the wires to be experimented on and for that purpose introduced into the copper cylinders.

In the brass tubes, *c c* and *c' c'*, through which the wires passed intended for the experiments, small disks of wood pierced with one hole were placed, and introduced far enough to be near the ends of the copper cylinders. The aperture in these disks was quite large enough for the passage of the thickest wires. They were for the purpose of cutting off the wires from conductive contact with the copper cylinders, and also to form a sort of bottom in the tubes. To make an hermetical closure around the wires the following process was adopted:—I first introduced, round the wire, a small flock of cotton, which was strongly pressed against the wooden disk; then I filled the tube with a melted mixture of wax and rosin. The cotton prevented the fused mixture from running into the cylinder. Of all the means I tried for hermetically sealing these tubes, this proved to be the best; in no one instance has the purpose not been attained. Before the union of the glass tube with the two brass tubes, I introduced therein, as index, an alcoholic mixture forming a column of a few centims. length. This index could be placed in a suitable position in the glass tube by turning the cocks so as to put the two extremities of the tube in communication with the external air and properly turning the apparatus about the pivots *e*. Before the commencement of each series of observations, I always ascertained that the tubes *c c'* and the caoutchouc tubes were hermetically closed by the mixture of wax and rosin: the examination took place in the following manner:—Before putting the glass tube in the place it was to occupy, one of the brass tubes was, by means of a caoutchouc tube, connected with a manometer consisting of a bent glass tube placed vertically and partly filled with water. I increased or diminished by some centimetres the height of the column of water in the open leg of the tube, so that the pressure of the air in the copper cylinder was slightly greater or less than that of the outer air. If the column of water remained the same during a sufficiently long time, it was a proof that the cylinder was hermetically closed. To ascertain the hermeticity of the caoutchouc tubes between the glass tubes and the two brass ones, I proceeded as follows:—One of the extremities of the glass tube was closed by means of one of the cocks, while the other extremity was put in communication with the external air by means of the other cock. If, on inclining the glass tube, the index remained motionless for a certain time, it was a proof of the hermeticity of the binding



caoutchouc tube. I afterwards ascertained, by the same procedure, the hermeticity of the joints at the other extremity of the glass tube.

Let us suppose now that a wire of two metals, A and B, soldered together, has been introduced into one of the cylinders, and an identical wire of the same metals and of the same thickness into the other, that the glass tube is fixed hermetically in its position, and that the tubes  $cc'$  are likewise hermetically closed. If now one and the same galvanic current passes through the wires of both cylinders (from metal A to metal B, for example), an equal quantity of heat is developed in the cylinders. For this reason the index in the glass tube remains at rest. It is evident that the same ought to be the case when the force of the current varies, and consequently that the development of heat occasioned by the resistance of the wires has no influence on the movement of the index. If, on the other hand, the wires are united to one another in such a manner that in one of the cylinders the current passes from A to B, and in the other from B to A, heat will be developed at one of the points of contact, and a cooling will take place at the other. The development of heat in the two cylinders will no longer be equal, and the index will move toward the cylinder presenting the less development of heat. This displacement will continue until the heating of each cylinder is equal to the cooling occasioned by radiation and by contact with the surrounding air. After three quarters of an hour no displacement of the index could be perceived; with the less delicate apparatus which I used in my first researches, this took place in a much shorter time. Once the index is at rest, the quantity of heat lost by the air of the cylinder by radiation and the contact of the external air is consequently equal to the quantity received from the wires by the air in the cylinder. The difference between the loss of heat in one cylinder and in the other is therefore found to be equal to the difference between the amounts of heat produced in the two wires; and, as will be demonstrated below, the former difference can be calculated from the amount of displacement of the index.

If any alteration take place in the temperature of the laboratory, it can have no effect on the motion of the index. The two copper cylinders are perfectly identical. Surrounded with jackets of polished zinc, which are both of the same size and shape and contain an equal quantity of water, they are placed at but a small distance one from the other. An alteration in the temperature of the laboratory acts but slowly on the copper cylinders; for it has first to traverse the considerable mass of water (5·8 kilogrammes) which each jacket contains. It was nevertheless shown, in the experiments, that the index had a motion proper

to itself, independent of the temperature at the points of soldering of the wires. It shifted slowly, and pretty regularly, from one cylinder toward the other; and this movement did not diminish or cease until the current had been in continual circulation during five or six hours. If a weaker current was made to follow an intense current which had circulated some hours in the wires, the displacement of the index was effected in the opposite direction. It was only after several experiments that I succeeded in discovering the cause of this strange phenomenon. In consequence of the action of the water on the zinc, one of the cylinders of this metal had become covered on the inside with a very thick and dense coating of hydrated oxide of zinc. Now this covering, being a very bad conductor of heat, prevented it from passing from the annular space round the cylinder of copper to the mass of water; consequently the temperature about the copper cylinder rose, and occasioned a continual increase of heat in the cylinder itself. The zinc jackets having been opened and cleaned, the inconvenience mentioned was, to a very great extent, removed. This shows how necessary it is that the jackets be in every respect perfectly equal the one to the other. The best procedure is, to coat their interior with a metal that will remain unaltered. As to the outer surfaces, it is easy to see to their remaining always alike.

It was easy, however, to eliminate this displacement of the index. I observed that during several hours it moved in one direction continually and with a tolerably uniform velocity. The experiments, besides, demonstrated that the displacement produced by the difference of temperature of the points of contact ceased in the space of about three quarters of an hour. Profiting by the above-mentioned circumstances, I always proceeded in the following manner in my experiments:—After the current had been in circulation for three quarters of an hour, I read on the scale the position of the index. This done, I reversed the current, and the index began to move in the opposite direction; at the end of another three quarters of an hour the position of the index was again read. The result of these two readings was a deviation  $a$ . I then restored to the current its first direction, and three quarters of an hour afterward I took a fresh reading; from this reading and the immediately preceding one I obtained deviation  $b$ . I next reversed the current once more; and from the last reading, treated in the same manner as the others, I obtained deviation  $c$ , in which the index was displaced in the same direction as in the first. Now, if we take the mean of  $a$  and  $c$ , and then the mean of this mean and  $b$ , we obtain the deviation sought. It is evident that this process furnishes a result entirely independent of the proper motion of the index, since this motion

is constant during the time of the observation, or its modification is proportional to the time. As a rule, several such determinations were made, in order to obtain a result so much more certain. But this made it very slow work, and involved a great loss of time.

The sensitiveness of the apparatus it is very easy to determine. It follows, from the dimensions above given of the copper cylinders, the diameter of the glass tube being 2·5 millims., that the volume of a millimetre length of the latter is to that of each copper cylinder as 1 to 12800. Supposing we had only one cylinder, and that the indicating tube fixed to it opened into the free air, it would be necessary, for a variation  $=t$  in the temperature of the enclosed air, that the displacement of the index should be such that

$$mv = 0\cdot00366t \cdot V,$$

in which  $V$  represents the volume of the cylinder,  $v$  that of a millimetre length of the tube, and  $m$  the number of divisions of the scale giving the displacement of the index.

Putting  $\frac{v}{V} = \beta = \frac{1}{12800}$ , we obtain  $m\beta = 0\cdot00366t$ . Of course this value is correct only as long as the pressure of the air remains constant. If now the copper cylinder be connected with another cylinder of the same volume, as was the case in the experiments, we shall have,  $H$  designating the pressure before, and  $h$  the pressure after the variation  $t$ ,

$$V(1 + 0\cdot00366t) \frac{H}{h} - V = mv = m\beta V.$$

The copper cylinders being of equal dimensions, and the temperature of one diminishing, on the reversal of the current, by a quantity equal to the augmentation of temperature in the other, we obtain further

$$h = H \frac{(1 - 0\cdot00366t)}{1 - m\beta}.$$

Substituting this value in the preceding equation, we obtain

$$0\cdot00366t = m\beta.$$

The deviation obtained with the combined cylinders will therefore be of the same amount as if only one cylinder were employed and the indicating tube were open to the external air, supposing the pressure of the latter constant. Making  $m=1$ , we obtain  $t=0^{\circ}\cdot002134$  Cels. A deviation of one division of the scale requires, therefore, a variation (in round numbers) of 0·002 degree in the temperature of the cylinder. Now it is evident that, if this deviation is to be obtained for the above-mentioned increase of temperature, the index must not be obstructed in its movement by friction, adhesion, &c. It was



therefore necessary to ascertain that the index was sufficiently free in this respect. Supposing it to move with perfect freedom, a difference of pressure of  $\frac{1}{128000}$  of an atmosphere would, according to what precedes, be sufficient to remove it one division of the scale. This quantity corresponds to a difference of pressure of 0.4 milligramme on the two extremities of the index. I satisfied myself in the following manner that an index composed of a mixture of alcohol and water really moves under this difference of pressure:—I used for the experiment an index of 2 centims. length. Its weight, taking 0.9 for the specific gravity of the mixture, was 88 milligrammes. Now, for the angle ( $x$ ) which the tube must make with the horizontal in order that in virtue of its own weight the index may receive in one of the directions a pressure of the amount above indicated, we evidently obtain  $88 \sin x = 0.4$ , whence  $x = 15'5$ . It was shown, at the time of the experiments, that the index commenced moving at an angle of inclination which was only a small fraction of the calculated angle. It may therefore be admitted that, with an index of this nature, the apparatus evidently indicated at least the half of a thousandth of a degree—a sensitiveness which, as special experiments demonstrated, was not diminished with an index of several centimetres length. On the other hand, an angle of inclination of several degrees was required for the commencement of motion of a sulphuric acid index of the same length; so the employment of this liquid could not be thought of. All the following experiments were made with an index of the alcoholic mixture above mentioned.

### § 3.

As we know, the cooling of a body in the air is not proportional to the excess of its temperature, but increases more rapidly than the latter. It is only when the excess is a minimum that the cooling can be regarded as proportional to it. In the following experiments, the excess of temperature of the *air* of the copper cylinders above the temperature of the zinc jacket, and above that of the air between the jacket and the cylinder was at the most 1 or 2 degrees; and consequently the excess of temperature of the cylinders themselves was still less. Let  $A$  be the quantity of heat which the copper cylinders lose by radiation and the contact of the external air,  $\tau$  the excess of temperature of the copper sides, and  $a_1$  and  $b_1$  constants, we may then put\*

$$A = a_1\tau + b_1\tau^2.$$

\* According to Dulong and Petit, the formula of cooling is

$$Ma^\Theta(a\tau - 1) + N\tau^{1.233},$$

where  $\Theta$  is the temperature of the surrounding bodies,  $\tau$  the excess of tem-

But  $\tau$ , or the excess of temperature of the sides of the copper cylinders, is unknown. The temperature of the air in the cylinders has, of course, its maximum in the vicinity of the wires, and its minimum near the sides of the cylinder. The temperature once in *equilibrium* (that is, as soon as the cylinders lose an equal quantity of heat to that generated by the current through the wires),  $\tau$  becomes a function determined from the mean excess of temperature of the air in the cylinders. This mean excess results from the two following causes:—1, the heat generated in the wires by the resistance to the passage of the current; 2, the variation of temperature at the surfaces of contact. We will call  $T$  the mean temperature which would be produced after the temperature has arrived at equilibrium, if the first of these causes acted alone; and we will designate by  $t$  the alteration occasioned by the second. For the case in which the two sources of heat reinforce one another, the mean excess of temperature above mentioned will therefore be expressed by  $T+t$ ; and for the case in which they are opposed, by  $T-t$ .

The excess of temperature,  $\tau$ , of the copper sides is therefore determined in the first case from  $T+t$ , and from  $T-t$  in the second. Now, if we suppose this function to be developed in a series according to the ascending powers of  $T+t$ , and that sufficient accuracy will be attained by keeping the first two

perature of the copper cylinders, and  $M$ ,  $N$ , and  $a$  constants. Taking 1 degree Celsius (Centigrade) as unit, we have, according to Dulong and Petit,  $a=1\cdot0077$ ; with a smaller unit,  $a$  of course becomes less also. Developing in series, and designating by  $k$  the natural logarithm of  $a$ , we obtain

$$\begin{aligned} a\Theta(a\tau-1) &= k\left(1+k\Theta+\frac{k^2\Theta^2}{2}+\frac{k^3\Theta^3}{2\cdot3}+\&c.\right)\tau \\ &+ \frac{k^2}{2}\left(1+k\Theta+\frac{k^2\Theta^2}{2}+\frac{k^3\Theta^3}{2\cdot3}+\&c.\right)\tau^2 \\ &+ \frac{k^3}{2\cdot3}\left(1+k\Theta+\frac{k^2\Theta^2}{2}+\frac{k^3\Theta^3}{2\cdot3}+\&c.\right)\tau^3+\&c., \end{aligned}$$

or, abbreviating,

$$a\Theta(a\tau-1)=kB\tau+\frac{k^2B\tau^2}{2}+\frac{k^3B\tau^3}{2\cdot3}+\&c.$$

Now  $k$  is very small; for  $a=1\cdot0077$  its value is  $0\cdot00767$ . Taking  $0\cdot001$  as unit, we have, as is easily demonstrated,  $k=0\cdot00000767$ , or one thousandth of its primitive value. This series is consequently rapidly convergent for values of  $\tau$  not too great; thanks to which circumstance, keeping two terms gives a sufficient approximation. As, further, the variations of  $\Theta$  are relatively small, we can, without too great an error, regard  $B$  as constant. In this way we obtain  $A=a_1\tau+b_1\tau^2$ , a formula which, by means of convenient values for the constants, may be regarded as including the term  $N\tau^{1\cdot233}$ .





pair of wires is exchanged for another; so that  $\beta$  must be determined for each pair by observation.

Now, if at two different current-intensities,  $s$  and  $s_p$ , the corresponding values  $t$  and  $t_p$  of the movement of the index be measured, we obtain, from equation (5),

$$\beta = \frac{(t_p s + t s_p)(t_p s - t s_p)}{s^2 \cdot s_p^2 (t + t_p)(t - t_p)} \dots \dots \dots (6)$$

The observations given further on have been calculated by means of equations (5) and (6).

As is evident from what precedes, the method of observation we have described is founded on the circumstance that, once the temperature is stationary, the cylinder loses a quantity of heat equal to that generated in the wires. In principle this method is correct. Nevertheless the formulæ by which the quantity of heat that disappears is calculated are only approximative; but the experiments demonstrate that the approximation is sufficient. As the interval from one observation to another was three quarters of an hour, the work took a considerable time. When the whole of the metallic combinations had thus been studied from both points of view, electromotive and thermoelectric, wishing, as a control, to determine the electromotive forces again, I proceeded in the following manner:—The current was reversed at the end of a quarter of an hour, during which the temperature of the cylinder had not time to acquire equilibrium. If now all the heat generated in the wire passed into the air enclosed in the cylinder, without any of it remaining in the wire or disappearing through the sides of the cylinder, an exact measure of the heat produced would be obtained by determining, from the displacement of the index, the increase of the mean temperature of the air. But this is not the case: a part of the heat remains in the wire; and a part passes into the sides. The quantity of heat which, remaining in the wire, occasions the heating of it, depends on the calorific capacity of the wire; but the whole amount of this is insignificant in comparison with that which passes into the copper sides. It is therefore quite unnecessary to take account of the difference, still more insignificant, between the quantities of heat remaining in the different pairs of wires. The part of the heat generated which thus does not pass into the air may be regarded as a function of the total augmentation of the temperature of the air during the interval of time, and may be expressed by the first two powers of that augmentation. This hypothesis must be verified by the observations themselves before it can be admitted as correct; as will be afterwards seen, the observations made furnish this verification. If  $\tau$  is the increase of the mean temperature of the air at the end of a quarter of an

hour, and  $a$ , a constant, the quantity of heat which has remained in the air is expressed by  $a\tau$ ; and the quantity which, at the expiration of the same space of time, has remained in the wire or has passed into the sides of the cylinder, by  $a_{II}\tau + b\tau^2$ , in which  $a_{II}$  and  $b$  are constants. If, then, we call the entire quantity of heat produced  $A_I$ , we have  $A_I = a\tau + b\tau^2$ . On reversing the current, we obtain, in the same manner,  $A_{II} = a\tau_I + b\tau_I^2$ . The difference  $W$  between these two values is the quantity sought, which will be

$$W = a(\tau - \tau_I) + b(\tau - \tau_I)(\tau + \tau_I).$$

Now  $\tau - \tau_I$  is indicated by the space moved through by the index during the time in question; and  $\tau + \tau_I$  is evidently the increase of temperature resulting from the quantity of heat produced in the wire in consequence of the galvanic resistance. Designating, as before, the first by  $t$ , and the second by  $T$ , we obtain

$$W = at + bTt,$$

which is identical with equation (3) above given. Thus, even when the observations follow at intervals of 15 minutes, the formulæ above given can be used for the calculation, although, perhaps, with modified values of the constants.

One observation following another, as has been said, every 15 minutes, it was found that, chiefly for the copper-tin combination, the successive deviations of the index varied considerably in amount—a circumstance which I could not at first account for. On closer reflection, however, its cause was easily discovered. As was said above, a quarter of an hour is not sufficient to render the temperature stationary. Let us suppose that with the same direction and the same intensity the current has taken a longer time to traverse the cylinders—three quarters, for example, or even more—and that then it is reversed, and its direction afterwards changed every quarter of an hour. In that cylinder in which heat was developed at the point of contact during the 45 minutes above supposed, the temperature of the copper wall is relatively high, and thence the cooling greater than in the other. As soon as the current is reversed, the air which surrounds the point of contact in the first cylinder cools; while the wall continues to lose a great quantity of heat, in consequence of its still comparatively elevated temperature; and in the second cylinder the source of heat is augmented, while the cooling is relatively very little. The index, therefore, has necessarily a considerable deviation in the direction of the first cylinder. After a quarter of an hour the current is again reversed before the sides of the first cylinder have had time to cool and those of the second to be heated to the degree corresponding

to the sources of heat existing in each cylinder. It may now easily happen, in the course of the observations, that the temperature of the sides is relatively low in the cylinder in which the point of contact commences to cool at the moment of the reversal of the current, and that the temperature of the sides of the other cylinder is relatively high at the same moment; the deviation of the index will then be much less than in the former case. It is consequently impossible to measure with certainty by a single observation the movement of the index; this measurement can only be obtained by taking the mean of a great number of observations.

The metals investigated were in the form of wires of about 1 millim. diameter, except those of bismuth, tin, and lead, which were thicker. The copper used had been precipitated by the galvanic method; and the metals bismuth, tin, lead, gold, zinc, and cadmium had been chemically purified from foreign elements. The silver likewise could be regarded as pure; for it contained only 0.01 per cent. of foreign matter. The iron contained 0.022 per cent. of carbon, 0.006 of silicium, 0.028 of phosphorus, and a slight trace of manganese, but was free from sulphur\*. In each combination, copper constituted one of the wires—with the exception of the pair containing palladium (which was soldered to a platinum wire), and that containing zinc (which was soldered to a silver wire). All the solderings were made with tin; the aluminium wire, however, had to be covered with a layer of copper, galvanically deposited, before the soldering could take place.

The intensity of the current was regulated by means of a rheostat, and measured by a tangent-compass. The circuit was furnished with the commutators required for the reversal of the current. Before the commencement of a series of observations, the current circulated in the wires during one or several hours, in order that the apparatus might have time to acquire equilibrium of temperature.

I pass now to the observations properly so called.

\* I owe the purification of these metals to the kindness of Professor Ekman and MM. Cleve and Wimmerstedt. I received the gold and silver from the royal mint, through M. Åkerman, the Director in Chief of that establishment, and the iron through Assistant Professor Åkerman. I had obtained the palladium from England, by the kindness of Mr. Nassau Jocelyn, Secretary of Legation of Her Britannic Majesty at the court of Stockholm.

[To be continued.]



XI. *On a Simple Case of Resonance.* By ROBERT MOON, M.A.,  
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I DESIRE to direct attention to a very simple instance of resonance, in the attempts to explain which a considerable amount of confusion appears to have arisen.

The case to which I refer is that described in Nos. 185, 186 of the article on Sound in the *Encyclopædia Metropolitana*†.

Suppose that a pipe closed at one end has at its opposite extremity a disk so placed as nearly to close it; and suppose that, by means of a tuning-fork or otherwise, the disk is made to vibrate in such a manner "that the performance of one complete vibration, going and returning, shall exactly occupy as much time as a sonorous pulse would take to traverse" a space equal to double the length of the pipe. The "first impulse" of the disk upon "the air will be propagated along the pipe and reflected at the stopped end, and will again reach the disk just at the moment when the latter is commencing its second impulse. But, the absolute velocity of the disk in its vibrations being excessively minute compared with that of sound, the reflected pulse will undergo a second reflection at the disk as if it were a fixed stopper. It will, therefore, in its return exactly coincide and conspire with the second original impulse of the disk; and the same process being repeated on every impulse, each will be combined with all its echoes, and a musical tone will be drawn forth from the pipe vastly superior to that which the disk vibrating alone in free air would produce."

Nothing can be more lucid, or apparently more convincing, than this explanation. Unfortunately the facts do not correspond with the conclusion to which the argument points.

If we are to accept the very precise and reiterated statements upon the subject of Professor Tyndall (Tyndall 'On Sound,' *ubi supra*), such an increase of tone as that above referred to occurs, not when the length of the pipe is half, but when it is one quarter of the length of a wave whose period of vibration, "going and returning," is the same as that of the disk; and if the above process of reasoning be applied to this altered state of circumstances, it will be found to lead to a conclusion precisely the opposite to that which has been above arrived at.

For, supposing the "first impulse" propagated by the disk into the pipe to consist of a condensation‡, such condensation will have entered the pipe, will have been reflected at the

\* Communicated by the Author.

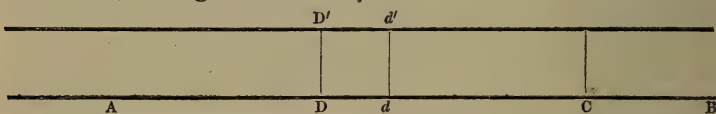
† See also Tyndall 'On Sound,' 1867, pp. 173-5.

‡ The adoption of the alternative supposition would lead to the same conclusion.

closed end thereof, and will be on the point of emerging from the open end and of undergoing reflection at the disk, at the precise moment that the latter is going to give birth to, and to propagate within the pipe, a rarefaction. These two disturbances, therefore, *i. e.* the twice reflected original condensation and the first rarefaction, will enter the pipe together, and will mutually destroy each other by interference. So far is it, therefore, from being true that, under the circumstances referred to, "the motion accumulates in the" pipe so as to produce a "vast augmentation of sound," the process of reasoning which has been adopted tends to show the exact contrary, *viz.* that there would be in the pipe immediately after the second impulse was concluded no motion whatever, and that the sonorous effect of the vibrating disk, so far from being enhanced, would be positively destroyed by the closure of the pipe.

There will be no difficulty, however, in explaining this remarkable phenomenon if we consider carefully the mode in which aërial waves are generated by the vibrating disk.

Suppose the pipe  $AB$  to be open at both ends, and that we have placed within it a closely fitting disk, represented by  $DD'$ , capable of sliding within the pipe; and let us consider the effect of the disk being removed from one position of rest,  $DD'$ , to another,  $dd'$ , during the interval  $t_1$ .



It is evident that at any instant during the first portion of  $t_1$  we shall have to the right of the disk a disturbance in the nature of a *condensation*, in which the velocity and condensation respectively will have their maximum values at the point of contact with the disk, and will thence gradually diminish as we recede to the right till they simultaneously vanish; while at the same instant we shall have on the left side of the disk a disturbance in the nature of a *rarefaction*, in which the velocity and rarefaction will in like manner be at a maximum at the point of contact with the disk, and will thence gradually diminish as we recede to the left till they simultaneously vanish.

The state of things just described will continue till the disk has attained its maximum velocity, at which epoch the air in contact with it on either side will also have its maximum velocity; the air to the right in contact with the disk having at the same time its maximum condensation, while that to the left has its maximum rarefaction\*.

Subsequent to this, at any instant during the remainder of  $t_1$ ,

\* The two disturbances will be symmetrical with respect to the disk in

the disturbances will be of this kind : viz. to the right the velocity and condensation will increase till each attains its maximum, and will thence gradually diminish till they vanish together ; while on the left hand the disturbance will be, *mutatis mutandis*, of precisely similar character.

At the end of the interval  $t_1$  we shall have on either side of the disk a complete wave, on the right of condensation, on the left of rarefaction, each capable of propagating itself without the aid of the disk, whose creative function with regard to each will be then finally closed.

It results from what has preceded, that at each instant of the interval  $t_1$  we shall have on the right of the disk, and acting upon it, a condensation, on the left a rarefaction—the difference of which will constitute a retarding force, which must have the effect of reducing the amplitude of vibration of the disk, and of reducing consequently the loudness of any sound which may be due to such vibration.

I now proceed to show that in the experiment before us the effect of closing the pipe at the point indicated by Professor Tyndall is, by detaining the ærial waves and returning them successively upon the disk at the proper moment, to destroy or reduce the retarding force exerted upon the disk by the surrounding air, so that, up to a certain limit, the amplitude of vibration of the disk, and with it the intensity of the sound resulting from its motion, will be increased at each succeeding excursion.

Let us now suppose the pipe to be closed by a fixed stopper at C, CD being equal to *half* the length of the wave of *condensation* produced by the motion in the time  $t_1$  of the disk from DD' to  $d d'$ , or equal to a *quarter* of the *mixed wave* of condensation followed by rarefaction which would be produced by the vibration of the disk in the time  $2t_1$  from DD' to  $d d'$  and *vice versa*.

The motion of the disk from DD' to  $d d'$  will have forced into the portion of the pipe to the right a *condensed* wave, which will be reflected at C, and at the end of the time  $t_1$  will be ready for reflection at the disk ; precisely at the moment when the latter, by its return movement from  $d d'$  to DD', is preparing to propagate into the portion  $dC$  of the pipe a rarefied wave.

Hence at any instant during the second interval  $t_1$  (in which the disk moves from  $d d'$  to DD') we shall have the following disturbances to the right of the disk, viz. :—

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this sense, viz. that at equal distances from the latter the excess above the mean density of the one will be equal to the defect below that density of the other, at the same time that the velocities are equal and in the same direction.



1. The portion of rarefied wave which the retreat of the disk towards  $DD'$  will have created.
2. The portion of the condensed wave before spoken of, which has already undergone reflection at the disk.
3. The remainder of the same condensed wave, *i. e.* the portion of it which has still to undergo such reflection.

Of these three superposed, or partially superposed disturbances—assuming the motion of the disk during the second interval  $t_1$  to be, except as regards direction, the same as in the first—the first two will destroy each other by interference, in the manner already explained, leaving the third as that which alone needs to be regarded in estimating the pressure exerted on the right face of the disk during the second interval  $t_1$ .

On the other hand, on the same assumption as to the motion of the disk, it is clear that the pressure on the left side of the disk at any instant during the second interval  $t_1$  will be that due to a *condensation*, and will be equal to the pressure at the same instant on the opposite side of the disk which would be due to the reflected portion of the condensed wave, supposing no such interference as before mentioned to have taken place—and therefore equal to the pressure actually exerted on the disk by the unreflected portion of the condensed wave, since the pressures on the disk of these two portions of the condensed wave are necessarily equal.

On the above assumption, therefore, as to the motion of the disk, it is clear that the pressures on the two sides of the disk at any instant during the second interval  $t_1$  would be equal and opposite, so that there would be an entire absence of any such force restraining the motion of the disk as, if the tube were left open, would necessarily exist.

It follows, therefore, that during the second interval  $t_1$  the disk will move through a longer space than it did during the first, and that this increased amplitude of vibration is due exclusively to the presence of the stopper at C.

During the next vibration of the disk (*i. e.* in its motion to the right during a *third* interval  $t_1$ ) similar effects will occur. In this case, however, the argument may be presented more simply in a somewhat different form.

If the closure of the tube had no effect in accelerating the motion of the disk during the second interval  $t_1$ , it is clear that at the beginning of the third interval  $t_1$  the air to the right of the disk would be at rest, since the whole of the original condensed wave would then have undergone reflection at the disk and have been destroyed by interference in the manner already explained. But by reason of the closure of the tube the excursion of the disk to the left during the second interval  $t_1$  will, as already

shown, have been increased; and the rarefaction which it leaves behind it will be increased in like manner; so that this latter disturbance (that is to say, the rarefaction to the right of the disk which its motion to the left tends to create) will not be entirely destroyed by interference with the original condensed wave; in other words, we shall have at the beginning of the third interval  $t_1$  a *rarefied* wave in position and circumstances precisely similar to the original condensed wave at the beginning of the second interval  $t_1$ ; *i. e.* by reflection at C it will have become doubled upon itself within the portion CD of the pipe, and will be on the point of undergoing reflection at the disk.

The reflection of this rarefied wave at the disk will have the effect of subjecting the latter at each instant of the third interval  $t_1$  to the influence of *two* rarefactions, the presence of each of which is entirely attributable to the occurrence of the stopper at C; viz. the rarefaction of the reflected portion, together with that also of the unreflected portion of the last-mentioned wave. The tendency of these will manifestly be to solicit the disk in the direction in which it is moving, and a further increase in the amplitude of excursion of the disk will necessarily take place.

The same reasoning will apply to the motion of the disk during successive intervals, till at last, supposing the impulses to be communicated to the disk by means of a tuning-fork, the increasing rigidity of the fork will prevent any further enlargement of its excursions; but in the mean time such an increased amplitude of excursion will have been produced as fully to account for the great increase of tone which takes place under the circumstances which we have been considering.

If it be asked why, in the case discussed by Sir John Herschel, the tendency to accumulation of vibrations in the closed tube does not produce the effects he supposed to result from it, it may be replied that, if the reasoning which I have applied to Professor Tyndall's case were adapted to Sir John Herschel's case, it would immediately appear that the stopper at C, by reflecting a wave back upon the disk, would have the effect of *diminishing* the excursion of the latter and the consequent loudness of the sound.

I may also observe that, if in Sir John Herschel's case all the accumulation took place which he supposed to occur, the loud sound spoken of could only be intermittent, prevailing at most during very brief intervals separated by much longer intervals of almost total silence—a variation in the phenomenon which, so far as I am aware, has not been observed to occur.

6 New Square, Lincoln's Inn,  
January 6, 1872.

XII. *On Glacier-motion.*By EDWARD VANSITTART NEALE, *Esq.**To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

IT appears to me that Canon Moseley has overlooked, in his valuable researches into the cause of the movement of glaciers, two considerations, both important, if I am not mistaken, in reference to that question, namely:—1st, the *accumulation of pressure* to which the lower part of a glacier must be exposed from the mass of ice behind tending to thrust it forwards; 2nd, the mode in which ice is formed.

1st. If we imagine a glacier to be divided, transversely, into sections of any assumed breadth, say one foot each, and suppose one of these sections placed on the bed of the glacier by itself, the force urging any particle of ice on the lower surface of the section forwards will, I conceive, be the weight of the whole mass of ice above and behind this particle, multiplied into the sine of the angle forming the slope of the glacier-bed. Now suppose a second section of equal breadth placed below the first, so that the upper face of this section shall be in contact with the lower face of the first section; the particles in this upper face must experience the same amount of forward thrust as those of the lower face with which they are in contact. Consequently the particles in the lower face of the second section will be subject to this thrust, *in addition* to that arising from the weight of the mass of ice in their own division; and thus, when any considerable length of ice is acting upon a slope, a pressure may be produced upon the particles of ice in its lowest extremity sufficient to overcome their adhesion to each other. Hence the glacier would yield in the line of least resistance—that is, in the direction of its length. The accumulated pressure of the rear ranks of particles would set the front ranks in motion, and these, in turn, would drag after them the particles in their rear by their molecular adhesion; so that a glacier might “*get under weigh*” simply by its own weight—though, if glaciers were masses of ice of definite extent, the result of this motion would probably be only to split it up into fragments where the retarding and impelling forces were evenly balanced, which would therefore again become stationary. But a glacier is *not* such a limited mass. It has an *unlimited reserve of power* in the snow of the mountain height whence it takes its origin, into which it gradually passes as its course is traced upwards. Its lower parts can never stop; for if they did, the store of power supplied by the freely moving masses of what in Savoy is called *névé* would



accumulate till the resistance was overcome. Hence the particles of glacier-ice, though to our senses they seem stationary, are always slowly moving, under the influence of the *thrust* behind them, gliding past the sides of their mountain-bed and past each other with velocities varying according to the direction and intensity of the lines of pressure acting upon them.

In regard to this motion, we must also bear in mind that the accelerative action of the particles moving in front of any given particle must always in great part counterbalance any retarding action of those moving at its sides; so that the accumulated pressure exerted by those behind it in order to keep up its movement would be limited to the *difference* between these two actions, which is probably small.

It appears, then, not difficult to account for the motions observed in glaciers by the accumulated effect of gravitation. But if the explanation here given is the true one, there ought to be other motions observable among the particles of a glacier, due to the impact of the bottom of the glacier on the irregularities of the surfaces over which it slides, which must, I conceive, produce lines of pressure *reflected* upwards obliquely from the bottom through the advancing mass. Accordingly we do find the evidence of such pressures in the *direction* of the ribboned structure often noticed in glacier-ice, the *thickness* of the bands being probably determined by the mass of *névé* consolidated into ice from year to year.

2nd. The force adduced in the preceding explanation appears sufficient to account for the movements of the particles of a glacier, even though their adhesion to each other should be as great as Canon Moseley's experiments indicate. But I cannot feel quite satisfied with the results obtained by him. We know that ice forms in thin layers, which lie one on the top of another. Canon Moseley's experiments on the *shearing-force* required to separate the particles of ice were made, I believe, by placing weights on the surface of a block of ice resting on a solid support, so that the weight would tend to compress the layers of ice and bring their particles into closer contact. Now the increase of molecular action appears to follow a ratio much above that of the inverse square. May not, therefore, the adhesive force of the particles of ice have been materially increased by the method used to ascertain it? If the motion of glaciers is to be subjected to strict mathematical reasoning, as is much to be desired, it seems to me that experiments are needed by which the adhesive force of ice shall be tested in a manner more nearly approaching the action of the pressure in a glacier upon it, *i. e.* by means of a force applied at *one end* of a block of ice held fast by lateral compression and its own weight; and until this has been done,

I much question whether we shall have any data truly applicable for calculating the amount of force required to produce the differential motions observed in glaciers.

I am yours &c.,

EDWARD VANSITTART NEALE.

Hampstead, January 8, 1872.

XIII. *A Contribution to the History of the Mechanical Theory of Heat.* By Professor CLAUSIUS.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

Bonn, January 6, 1872.

THERE has recently appeared a very valuable book, entitled "Theory of Heat, by J. Clerk Maxwell," in which the mechanical theory especially is regarded with favour, and many quotations and historical notices in reference to its origin and development are given. In all these details, however (with the exception of the "Molecular Constitution of Bodies"), my writings are left quite unmentioned; and my name occurs only once, when it is said that I introduced the word *entropy*; but it is added that the theory of entropy had already been given by W. Thomson. Hence any one who derives his knowledge of the matter solely from this book must conclude that I have contributed nothing to the development of the mechanical theory of heat. Since Professor Maxwell, through his many beautiful investigations, justly enjoys a wide-spread reputation, I think that, however reluctantly I resolve to do so, it is yet incumbent on me to say something in order to correct the representation he has given. Considering that you have done me the honour to admit into the Philosophical Magazine nearly all my memoirs on the mechanical theory of heat, I trust that also to these personal observations, to which I am compelled, you will kindly grant a place in your esteemed journal.

I remain, Gentlemen,

Most respectfully yours,

R. CLAUSIUS.

For the better understanding of the following, it will be useful first to say something on the condition of the theory of heat at the time of the publication of my first memoir relative thereto.

In the years 1842-49 had appeared the first splendid writings of Mayer, Colding, and Helmholtz on the conservation of energy, and a portion of the celebrated investigations of Joule on the mechanical equivalent of heat, but at that time had not become so well known as they deserved to be.

Then, in 1849, W. Thomson published his interesting memoir, "An Account of Carnot's Theory of the Motive Power of Heat, with Numerical Results deduced from Regnault's Experiments on Steam"\*. In this treatise he still adheres entirely to Carnot's view, that heat can perform work without the quantity of heat present being altered. It is true, he adduces a difficulty which opposes this view, and says (p. 545), "It might appear that the difficulty would be entirely avoided by abandoning Carnot's fundamental axiom—a view which is strongly urged by Mr. Joule." But he adds, "If we do so, however, we meet with innumerable other difficulties, insuperable without further experimental investigation and an entire reconstruction of the theory of heat from its foundation. It is in reality to experiment that we must look, either for a verification of Carnot's axiom and an explanation of the difficulty we have been considering, or for an entirely new basis of the theory of heat."

At the time of the appearance of this memoir, I wrote my first on the mechanical theory of heat, which was read in the Berlin Academy in February 1850, and was printed in the March and April numbers of Poggendorff's *Annalen*, and of which an English translation appeared in the *Phil. Mag.* S. 4. vol. ii.

In it I ventured to commence that reconstruction of the theory of heat, without waiting for further experimental investigation; and therein, I believe, I so far overcame the difficulties mentioned by Thomson that the path was smoothed for all further investigations of this sort.

In the first part of the memoir, which treated of the *theorem of the equivalence of heat and work*, I showed, first, that several quantities occurring in the science of heat required a conception and treatment quite different from those which they had hitherto received.

The amount of heat which a body must receive in order, from a given initial condition, to arrive at another, and which was named the *total heat* of the body, had previously been universally treated as a quantity which was perfectly determined by the momentary state of the body; and accordingly it was represented by a function of the volume and temperature, or of the volume and pressure. I now showed that such a mode of representation is inadmissible, this quantity depending not merely on the momentary state of the body, but also on the way in which it has arrived at that state.

Of the so-called latent heat, I maintained that it no longer exists as heat, but has been expended for the production of work. I distinguished the work into *internal* and *external*, and made evident an essential difference between the two—the former being

\* Transactions of the Royal Society of Edinburgh, vol. xvi. p. 541.



independent of the mode of the alterations, while the latter is dependent thereon.

The heat expended for internal work I united with the heat actually present in the body into one quantity, which has the character previously assumed to be that of the total heat, viz. that it may be represented by a function of the volume and temperature. To this function, which I denoted by  $U$ , Thomson subsequently gave the very suitable name of *energy* of the body.

In the second part of the memoir, which related to Carnot's theorem, I showed that, in the form in which Carnot expressed it, this theorem cannot be correct—and, further, that Carnot's proof of it (resting on the axiom that *it is impossible to create moving force out of nothing*) is, after the acceptance of the above-mentioned first theorem, no longer tenable. On the other hand, I showed, further, that, by a certain modification of Carnot's theorem, its agreement with the first theorem can be restored; and I demonstrated the theorem, thus modified, by admitting a new axiom. This axiom, in its briefest form, is:—*Heat cannot of itself pass from a cooler into a hotter body.*

After the establishment of the general notions, I applied the two theorems to permanent gases and to the process of evaporation.

Among other results, the application to gases gave the first reliable determination of Carnot's temperature-function; for an earlier determination, made by Holtzmann, rested on calculations demonstrably inaccurate, and hence incapable of affording any guarantee for the correctness of the result.

The application to the process of evaporation led to two important consequences:—(1) that steam, when it expands without the introduction of heat, and overcoming a resistance corresponding to its full force, must therein be partially precipitated, and, hence, that the specific heat of saturated steam is a *negative* quantity; and (2) that saturated steam does not follow Mariotte and Gay-Lussac's law, as in all previous calculations had been presupposed, but deviates greatly from it. The new equations which I already in that first memoir constructed for the nearer determination of the behaviour of vapours, are at the present time still recognized as perfectly correct, and are universally employed.

In the same month (February 1850) in which my memoir was brought before the Berlin Academy, a very valuable memoir, by Rankine, was brought before the Royal Society of Edinburgh, and was published in the Transactions of that Society\*.

Therein Rankine advances the hypothesis that heat consists in

\* Vol. xx. p. 147. In 1854 it was reprinted, with some alterations, in the Phil. Mag. S. 4. vol. vii. pp. 1, 111, & 172.

a whirling motion of the molecules, and very skilfully deduces from it a series of theorems on the behaviour of heat. It must particularly be mentioned that he also found the specific heat of saturated steam to be a negative quantity. He was unable, however, to effect so accurate a quantitative determination of this quantity as I, because at that time he still assumed for saturated steam the validity of Mariotte and Gay-Lussac's law.

The *second* theorem of the mechanical theory of heat was not treated by Rankine in this memoir, but first in another, which was brought before the Edinburgh Royal Society a year later (April 1851)\*. He there says himself, speaking of my handling of the theorem†, "and I had at first doubts as to the soundness of the reasoning by which he maintained it. Having stated those doubts to Professor Thomson, I am indebted to him for having induced me to investigate the subject thoroughly." He then added that he had now come to the conclusion that this theorem was not to be regarded as an independent principle in the theory of heat, but that it could be derived as a consequence from the equations which had been given in his earlier memoir.

He then communicates the new proof of the theorem. But (as I afterwards showed‡, without any contradiction from Mr. Rankine) this proof agrees with his own views on specific heat only where the body in question retains its state of aggregation. On the contrary, where alterations in the state of aggregation occur (and these are the most important cases), the proof contradicts the views which he had formerly expressed and which he subsequently maintained§.

Rankine added the memoir of 1851 as a fifth section to his previous memoir, on account of the affinity of the contents. Thence arose the error in some authors—who have written as if this new memoir had from the first formed part of the earlier memoir, and therefore Rankine and I had simultaneously given a demonstration of the second theorem of the mechanical theory of heat. But it is evident from the foregoing, that his demonstration (leaving out of consideration how far it is satisfactory) was first given a year after mine.

Likewise in 1851 (in March) a second memoir by W. Thomson on the theory of heat was laid before the Edinburgh Royal Society||. In this memoir he abandons his former position in relation to Carnot's theory, and adopts my conception of the

\* Edinb. Trans. vol. xx. p. 205. Phil. Mag. S. 4. vol. vii. p. 249.

† Phil. Mag. S. 4. vol. vii. p. 251.

‡ Pogg. Ann. vol. cxx. p. 434; and 'Mechanical Theory of Heat,' by R. Clausius, edited by T. Archer Hirst, p. 273.

§ Phil. Mag. S. 4. vol. xxx. p. 410.

|| Edinb. Trans. vol. xx. p. 261; reprinted in Phil. Mag. S. 4. vol. iv. pp. 8, 105, & 168.

second theorem of the mechanical theory. At the same time he amplified the considerations: while, in the mathematical treatment of the subject, I confined myself to the consideration of gases and the process of evaporation, merely adding that it would be easily seen how corresponding applications to other cases also could be made, Thomson developed a series of more general equations independent of the state of aggregation of the bodies, and then passed on to more special applications.

In one point, however, this later memoir also remained behind my own. That is to say, here also he adheres to Mariotte and Gay-Lussac's law in the case of saturated steam, while he hesitates about an hypothesis relative to permanent gases which I had made use of in my developments. On this point he says\*:—"I cannot see that any hypothesis, such as that adopted by Clausius fundamentally in his investigations on this subject, and leading, as he shows, to determinations of the densities of saturated steam at different temperatures, which indicate enormous deviations from the gaseous laws of variation with temperature and pressure, is more probable, or is probably nearer the truth, than that the density of saturated steam does follow these laws as it is usually assumed to do. In the present state of science it would perhaps be wrong to say that either hypothesis is more probable than the other."

Several years later, after he had convinced himself, by experiments which he and Joule made in common, of the correctness of the hypothesis adopted by me, within the limits already indicated by myself, he also, for the first time, employed the same procedure as I for the determination of the densities of saturated steam†.

The position above stated has always, so far as I know, been most readily acknowledged by Messrs. Rankine and Thomson as that occupied in relation to one another by our first writings on the mechanical theory of heat. Thomson, in his memoir of 1851‡, says:—"The whole theory of the motive power of heat is founded on the two following propositions—due respectively to Joule, and to Carnot and Clausius." Accordingly he proceeds to cite the second theorem of the mechanical theory of heat under the designation of "Prop. II. (Carnot and Clausius)." Then, after communicating a demonstration discovered by himself of this proposition, he continues§:—"It is with no wish to claim priority that I make these statements, as the merit of first establishing the proposition upon correct principles is entirely

\* Edinb. Trans. vol. xx. p. 277; and Phil. Mag. S. 4. vol. iv. p. 111.

† Phil. Trans. 1854.

‡ Edinb. Trans. vol. xx. p. 264; and Phil. Mag. S. 4. vol. iv. p. 11.

§ *Ll. cc.* pp. 266 et 14.



due to Clausius, who published his demonstration of it in the month of May last year, in the second part of his paper on the Motive Power of Heat."

In spite of these utterances by W. Thomson, Mr. Maxwell has thought proper to leave my writings unmentioned. He circumstantially explains (pp. 145-155) that, in an essential point, Carnot was in error in his considerations, and that consequently the proposition expressed by him, as well as his demonstration of it, required to be altered; yet in all this he cannot find a single word in order to state who it was that made those alterations, and correctly expressed the second theorem of the mechanical theory of heat, and reduced it to true principles.

Besides, W. Thomson, in his memoir, when speaking of my demonstration, says\*:—"The following is the axiom on which Clausius's demonstration is founded:—*It is impossible for a self-acting machine, unaided by external agency, to convey heat from one body to another at a higher temperature.*" The proposition here printed in italics is in Mr. Maxwell's book (p. 153) quoted in exactly the same words in which Thomson has clothed it; but here, instead of the introductory words. "The following is the axiom on which Clausius's demonstration is founded," we have:—"Carnot expresses this law as follows." Thus, while in the remainder of the quotation Thomson's words are used, my name is replaced by that of Carnot, without a word of explanation for the alteration. This is so mysterious to me, that I cannot help supposing there must be a misprint. However, of course I must leave it to Mr. Maxwell to clear up the matter.

Out of the detailed statements which follow the above, I will only call attention to two passages.

The result above mentioned, and, on account of its importance, often spoken of by others, viz. that saturated steam has a negative specific heat, is cited by Maxwell in the following manner:—"It appears, from the experiments of M. Regnault, as shown in the diagram at p. 135, that heat leaves the saturated steam as its temperature rises, so that its specific heat is *negative*."

In reference to my way of calculating the density of saturated steam, from which have resulted considerable deviations from Mariotte and Gay-Lussac's law, and which was first adopted by Rankine and Thomson much later, Maxwell says:—"In the meantime Rankine has made use of the formula [the same that I have used] in order to calculate the density of saturated steam."

An *intention* on the part of Mr. Maxwell to suppress my name could, I think, hardly be more distinctly apparent than in these passages.

All the quotations above given referred to my first memoir on

\* *Ll. cc.* pp. 266 et 14.

the mechanical theory of heat (1850). I have since published, besides those which refer to electricity and to the molecular constitution of bodies, eight memoirs on that theory, all of which have been translated into English, and since 1867 combined in one work\*; but all of these have been left by Mr. Maxwell as unnoticed as the first.

To enter into the contents of these later memoirs would carry us too far. I will only permit myself to touch on one other point, viz. the theory which refers to the dissipation of energy, or to entropy, because another English author also, whom I esteem highly, Mr. Tait, in his 'Sketch of Thermodynamics,' attributes this theory to W. Thomson alone.

As already mentioned, I demonstrated Carnot's theorem, modified, thus:—I laid it down as a fundamental property, founded in the essential nature of heat, that it everywhere exhibits the tendency to compensate existing differences of temperature, and consequently to pass from warmer into cooler bodies, but can never, without compensation, pass from cooler into warmer bodies. There results from my considerations, as a corresponding difference, that the transformation of heat into work is only possible with compensation, while the transformation of work into heat can be effected without compensation.

Two years later a short article appeared, by Thomson, "On a Universal Tendency in Nature to the Dissipation of Mechanical Energy"†, in which he states the three following propositions as conclusions which he had drawn:—

1. "There is at present in the material world a universal tendency to the dissipation of mechanical energy."

2. "Any restoration of mechanical energy, without more than an equivalent of dissipation, is impossible in inanimate material processes, and is probably never effected by means of organized matter, either endowed with vegetable life or subjected to the will of an animated creature."

3. "Within a finite period of time past the earth must have been, and within a finite period of time to come the earth must again be, unfit for the habitation of man as at present constituted, unless operations have been, or are to be performed, which are impossible under the laws to which the known operations going on at present in the material world are subject."

These propositions were certainly worthy of admiration for their boldness and universality; but after what had preceded, it could not be said that they contained a new principle. Thomson himself designates them as "consequences which follow from

\* The Mechanical Theory of Heat. By R. Clausius. Edited by T. Archer Hirst. London: J. Van Voorst.

† Proc. Roy. Soc. Edinb. April 1852; and Phil. Mag. S. 4. vol. iv. p. 304.

Carnot's proposition . . . established, on a new foundation, in the dynamical theory of heat."

Very soon after the publication of those propositions, appeared an essay by Rankine "On the Reconciliation of the Mechanical Energy of the Universe"\*, wherein a view was stated which is in opposition to them, namely that by concentration of the heat-rays the lost differences of temperature might be restored. I have shown, however, in a special memoir†, that, even by radiation, under no circumstances can heat pass from a cooler into a warmer body and thereby increase an existing difference of temperature.

In other memoirs I have endeavoured to reduce to a definite quantity the universal tendency in nature to transformation, the value of which quantity can only alter in one direction and not in the contrary. In a memoir in 1854‡ I introduced a quantity,  $N$ , relative to any circular processes, naming it the *uncompensated transformation*, and defining it by the equation

$$N = \int \frac{dQ}{T},$$

in which  $dQ$  denotes a heat-element given out by a variable body to a heat-reservoir, and  $T$  its absolute temperature. I showed that the quantity  $N$  can only be *positive*, while 0 forms the limiting value relative to *reversible* circular processes.

In a paper that followed soon after, on Steam-engines, I made use of the same quantity, in order to obtain—instead of the usual determination of the work of a steam-engine, in which the quantities of work done during the various processes are singly determined and then added up—an opposite method, in which we start from the maximum of work, and then deduct from it the loss of work occasioned by the imperfections of the process (incomplete expansion, vicious space, less vapour-pressure in the cylinder than in the boiler, &c.). The amount of heat represent-

ing this loss of work I exhibited by the product  $T_0 N$  or  $T_0 \int \frac{dQ}{T}$ , where  $T_0$  signifies a temperature (occurring in the process) at which heat is given out§.

The very same formula is developed by Tait in his 'Sketch of Thermodynamics,' p. 100; but instead of quoting it as mine, he says:—"This is *Thomson's* expression for the amount of heat

\* Phil. Mag. S. 4. vol. iv. p. 358.

† Pogg. Ann. vol. cxxi. p. 1; and Mechanical Theory of Heat, p. 290.

‡ Pogg. Ann. vol. xciii. p. 499; Phil. Mag. S. 4. vol. xii. p. 81; Mechanical Theory of Heat, p. 127.

§ Pogg. Ann. vol. xcvii. p. 452; Phil. Mag. S. 4. vol. xii. p. 241; Mechanical Theory of Heat, p. 146.



*dissipated* during the cycle.” The place which he cites as that in which Thomson had given this expression is the above-mentioned article “On a Universal Tendency” &c.\* But in that article there is neither the above-quoted nor any equivalent expression; in it altogether only four formulæ occur, which are totally different. Therefore Mr. Tait’s assertion and the accompanying citation are to me inexplicable.

In a later memoir† I carried the considerations to still greater completeness. The above proposition on uncompensated transformations, which by omitting the symbol N can be written

$$\int \frac{dQ}{T} \geq 0,$$

referred only to circular processes. I now endeavoured to obtain a quantity valid for any alteration of a body, and whose value can only change in one direction. For this purpose, to the hitherto considered two sorts of transformation (viz. the transformation of work into heat and *vice versa*, and the passage of heat from a warmer into a cooler body and *vice versa*) I added a third relative to the change of state of a body, and represented by the quantity Z; this I named the *disgregation* of the body. With the aid of this quantity and H, denoting the heat actually present in the body, I was able, in place of the above relation, to construct the following more general one:—

$$\int \frac{dQ + dH}{T} + \int dZ \geq 0 \ddagger.$$

It was the sum

$$\int \frac{dH}{T} + \int dZ$$

for which I introduced the name *entropy* of the body§. By applying this new idea I was able to express the tendency of nature to transformation, more completely and definitely than it had hitherto been expressed by any one, in the short proposition:—*The entropy of the universe tends to a maximum.*

From the foregoing it will be sufficiently evident that the theory of the dissipation of energy, or entropy, was not developed by Thomson alone, but that I had an essential share in its development. How novel my treatment of the subject was, compared

\* Phil. Mag. S. 4. vol. iv. p. 304; and Proc. R. S. Edinb. 1852.

† Pogg. Ann. vol. cxvi. (1862) p. 73; Phil. Mag. S. 4. vol. xxiv. pp. 81 & 201; Mechanical Theory of Heat, p. 215.

‡ Pogg. Ann. vol. cxvi. p. 109; Mechanical Theory of Heat, p. 247.

§ Pogg. Ann. vol. cxxv. (1865) p. 390; Mechanical Theory of Heat, p. 357.

with every thing up to that time existing, is clear *e. g.* from this, that Tait, in his work (p. 111), when speaking of the memoir above mentioned, in which I amplified the considerations, says :—  
“Clausius has adopted an extremely different mode of attacking questions as to the effect produced by heat upon a substance.”

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XIV. *On the Influence of Gas- and Water-pipes in determining the Direction of a Discharge of Lightning.* By HENRY WILDE, Esq.\*

ALTHOUGH the invention of the lightning-conductor is one of the noblest applications of science to the wants of man, and its utility has been established in all parts of the world by the experience of more than a century, yet a sufficient number of instances are recorded of damage done by lightning to buildings armed with conductors to produce in the minds of some an impression that the protective influence of lightning-conductors is of but questionable value.

The destruction, by fire, of the beautiful church at Crumpsall, near Manchester, during a thunderstorm on the morning of the 4th instant, has induced me to bring before the Society, with a view to their being known as widely as possible, some facts connected with the electric discharge which have guided me for some years in the recommendation of means by which disasters of this kind may be averted.

For the proper consideration of this subject, it is necessary to make a distinction between the mechanical damage which is the direct effect of the lightning stroke, and the damage caused indirectly by the firing of inflammable materials which happen to be in the line of discharge.

Instances of mechanical injury to buildings not provided with conductors are still sufficiently numerous to illustrate the terrific force of the lightning stroke, and at the same time the ignorance and indifference which prevail in some quarters with respect to the means of averting such disasters; for wherever lofty buildings are furnished with conductors from the summit to the base and thence into the earth, damage of the mechanical kind is now happily unknown.

Even in those cases where lightning-conductors have not extended continuously through the whole height of a building, or where the lower extremity of the conductor has, from any cause, terminated abruptly at the base of the building, the severity of the stroke has been greatly mitigated, the damage being limited in many cases to the loosening of a few stones or bricks.

\* From the Proceedings of the Literary and Philosophical Society of Manchester, January 9, 1872. Communicated by the Author.

The ever extending introduction of gas- and water-pipes into the interior of buildings armed with lightning-conductors has, however, greatly altered the character of the protection which they formerly afforded ; and the conviction has been long forced upon me that, while buildings so armed are effectually protected from injury of the mechanical kind, they are more subject to damage by fire.

The proximity of lightning-conductors to gas- and water-mains, as an element of danger, has not yet, so far as I know, engaged the attention of electricians ; and it was first brought under my notice at Oldham in 1861, by witnessing the effects of a lightning discharge from the end of a length of iron wire rope, which had been fixed near to the top of a tall factory chimney, for the purpose of supporting a long length of telegraph-wire. The chimney was provided with a copper lightning-conductor terminating in the ground in the usual manner. In close proximity to the conductor and parallel with it the wire rope descended, from near the top of the chimney, for a distance of 100 feet, and was finally secured to an iron bolt inserted in the chimney about 10 feet from the ground. During a thunderstorm which occurred soon after the telegraph-wire was fixed, the lightning descended the wire rope, and, instead of discharging itself upon the neighbouring lightning-conductor, darted through the air for a distance of 16 feet to a gas-meter in the cellar of an adjoining cotton warehouse, where it fused the lead-pipe connexions and ignited the gas. That the discharge had really passed between the end of the wire rope and the lead-pipe connexions was abundantly evident from the marks made on the chimney by the fusion and volatilization at the end of the wire rope and by the fusion of the lead pipe. As the accident occurred in the daytime, the fire was soon detected and promptly extinguished.

Another and equally instructive instance of the inductive influence of gas-pipes in determining the direction of the lightning discharge occurred in the summer of 1863, at St. Paul's Church, Kersal Moor, during divine service. To the outside of the spire and tower of this church a copper lightning-conductor was fixed, the lower extremity of which was extended under the soil for a distance of about 20 feet. The lightning descended this conductor, but, instead of passing into the earth by the path provided for it, struck through the side of the tower to a small gas-pipe fixed to the inner wall. The point at which the lightning left the conductor was about 5 feet above the level of the ground, and the thickness of the wall pierced was about 4 feet ; but beyond the fracture of one of the outer stones of the wall and the shattering of the plaster near the gas-pipe, the building sustained no injury.



That the direction of the electric discharge had in this case been determined by the gas-pipes which passed under the floor of the church, was evident from the fact that the watches of several members of the congregation who were seated in the vicinity of the gas-mains were so strongly magnetized as to be rendered unserviceable.

The church at Crumpsall is about a mile distant from that at Kersal Moor; and the ignition of the gas by lightning, which undoubtedly caused its destruction, is not so distinctly traceable as it is in other cases which have come under my observation, because the evidences of the passage of the electric discharge have been obliterated by the fire. From information, however, communicated to me by the clerk in charge of the building as to the arrangement of the gas-pipes, the most probable course of the electric discharge was ultimately found.

The church is provided with a copper lightning-conductor, which descends outside the spire and tower as far as the level of the roof. The conductor then enters a large iron down-spout, and is carried into the same drain as that in which the spout discharges itself. Immediately under the roof of the nave and against the wall, a line of iron gas-pipe extended parallel with the horizontal lead gutter which conveyed the water from the roof to the iron spout in which the conductor was enclosed. This line of gas-piping, though not in use for some time previous to the fire, was in contact with the pipes connected with the meter in the vestry, where the fire originated, and was not more than three feet distant from the lead gutter on the roof. As no indications of the electric discharge having taken place through the masonry were found, as in the case of the church at Kersal Moor, it seems highly probable that the lightning left the conductor at the point where the latter entered the iron spout, and by traversing the space between the leaden gutter and the line of gas-piping in the roof found a more easy path to the earth by the gas-mains than was provided for it in the drain.

In my experiments on the electrical condition of the terrestrial globe\*, I have already directed attention to the powerful influence which lines of metal, extended in contact with moist ground, exercise in promoting the discharge of electric currents of comparatively low tension into the earth's substance, and also that the amount of the discharge from an electromotor into the earth increases conjointly with the tension of the current and the length of the conductor extended in contact with the earth. It is not, therefore, surprising that atmospheric electricity, of a tension sufficient to strike through a stratum of air several hundred yards thick, should find an easier path to the earth by leaping

\* Philosophical Magazine, August 1868.

from a lightning-conductor through a few feet of air or stone to a great system of gas- and water-mains, extending in large towns for miles, than by the short line of metal extended in the ground which forms the usual termination of a lightning-conductor.

It deserves to be noticed that in the cases of lightning discharge which I have cited, the lightning-conductors acted efficiently in protecting the buildings from damage of a mechanical nature, the trifling injury to the church tower at Kersal Moor being directly attributable to the presence of the gas-pipe in proximity to the conductor. Nor would there have been any danger from fire by the ignition of the gas if all the pipes used in the interior of the buildings had been made of iron or brass instead of lead; for all the cases of the ignition of gas by lightning which have come under my observation have been brought about by the fusion of lead pipes in the line of discharge. The substitution of brass and iron, wherever lead is used in the construction of gas-apparatus, would, however, be attended with great inconvenience and expense, and moreover would not avert other dangers incident to the disruptive discharge from the conductor to the gas- and water-pipes within a building. I have therefore recommended that in all cases where lightning-conductors are attached to buildings fitted up with gas- and water-pipes, the lower extremity of the lightning-conductor should be bound in good metallic contact with one or other of such pipes outside the building. By attending to this precaution the disruptive discharge between the lightning-conductor and the gas- and water-pipes is prevented, and the fusible metal pipes in the interior of the building are placed out of the influence of the lightning discharge.

Objections have been raised by some corporations to the establishment of metallic connexion between lightning-conductors and gas-mains, on the ground that damage might arise from ignition and explosion. These objections are most irrational, as gas will not ignite and explode unless mixed with atmospheric air, and the passage of lightning along continuous metallic conductors will not ignite gas even when mixed with air. Moreover, in every case of the ignition of gas by lightning, the discharge is actually transmitted along the mains, such objections notwithstanding. A grave responsibility therefore rests upon those who, after introducing a source of danger into a building, raise obstacles to the adoption of measures for averting this danger.

XV. *Electrodynamic Measurements.* By Professor WILHELM WEBER.—Sixth Memoir, relating specially to the Principle of the Conservation of Energy.

[Concluded from p. 20.]

8. *On the Movement of two Electrical Particles in consequence of their action on each other.*

THE fundamental electrical law determines the action exerted by any given particle upon another under any circumstances. The simplest and most obvious application that can be made of this law, would seem to be to develop the laws of the motion of two particles which act mutually upon each other. Greater practical interest, however, attached to the determination, in the first place, of the laws of the distribution of electricity at rest upon conductors, and of the laws of the forces exerted by a current of electricity in a closed conductor, by reason of the current existing in another conductor, upon this latter conductor itself—as well as to the development of the laws of the (electromotive) forces exerted by closed currents (or by magnets) on the electricity in closed conductors—inasmuch as the results of these developments admitted of being directly tested and confirmed by experiment. But although this important practical interest is wanting to the development of the laws of motion of two particles subject only to their mutual action, many of its results cannot fail to merit attention in other respects.

The interest which belongs to these results relates indeed specially to the *molecular movements* of two particles, movements which are shut out from all direct experimental investigation, so that there is no authority for the application to them of the law that has been established, so far as it is regarded as an experimental law. Consequently the development of the laws of the *molecular movements* of two particles in accordance with the law that has been established must be considered only as an attempt to find a clue to the theory (which as yet we are entirely without) of these movements—a clue which by itself is certainly not sufficient, but is still in need of being supplemented in essential respects. For so long as the *molecular forces acting only at molecular distances*, which doubtless cooperate in the molecular movements, are not known and taken exact account of, the results that may be acquired cannot have any exact *quantitative* application, but only a *qualitative* value within certain limits, and can be of consequence only for a first *reconnaissance* of the territory.

9. *Motion of two Electrical Particles in the direction of the straight line which joins them.*

For two particles,  $e$ ,  $e'$ , moving simply in consequence of their



mutual action, we have, according to the fundamental laws of Section 4, by putting

$$\rho = 2 \left( \frac{1}{\epsilon} + \frac{1}{\epsilon'} \right) \frac{ee'}{cc}, \quad x = \frac{1}{2} \frac{\epsilon\epsilon'}{\epsilon + \epsilon'} \cdot \frac{dr^2}{dt^2}, \quad a = \frac{1}{2} \frac{\epsilon\epsilon'}{\epsilon + \epsilon'} \cdot cc$$

and also giving a negative sign to  $U$  and  $V$ , so as to denote thereby the *potentials*,

$$V : U = 2 \left( \frac{1}{\epsilon} + \frac{1}{\epsilon'} \right) \frac{ee'}{cc} : r \\ -U + \frac{1}{2} \frac{\epsilon\epsilon'}{\epsilon + \epsilon'} \cdot \frac{dr^2}{dt^2} = \frac{1}{2} \frac{\epsilon\epsilon'}{\epsilon + \epsilon'} \cdot cc;$$

and therefore

$$V = \frac{2}{r} \left( \frac{1}{\epsilon} + \frac{1}{\epsilon'} \right) \frac{ee'}{cc} \cdot U = \frac{ee'}{r} \left( \frac{1}{cc} \cdot \frac{dr^2}{dt^2} - 1 \right).$$

If there is no *motion of rotation of the particles about each other* in space,  $\frac{1}{\epsilon} \cdot \frac{dV}{dr}$  is the acceleration of the particle  $e$  in the direction of  $r$ , and  $\frac{1}{\epsilon'} \cdot \frac{dV}{dr}$  is the acceleration of the particle  $e'$  in the opposite direction. Hence the *relative acceleration* of the two particles becomes

$$\frac{ddr}{dt^2} = \left( \frac{1}{\epsilon} + \frac{1}{\epsilon'} \right) \frac{dV}{dr};$$

and from this, by integrating between the limits  $r=r_0$  and  $r=r$  ( $r_0$  denoting the value of  $r$  for the moment when  $\frac{dr}{dt}=u=0$ ),

since  $\rho$  was made  $= 2 \left( \frac{1}{\epsilon} + \frac{1}{\epsilon'} \right) \frac{ee'}{cc}$ , we obtain

$$\frac{dr^2}{dt^2} = uu = \frac{r-r_0}{r-\rho} \cdot \frac{\rho}{r_0} \cdot cc.$$

$\frac{\rho}{r_0}$  has always a positive or negative value differing from nothing; for  $\rho = 2 \left( \frac{1}{\epsilon} + \frac{1}{\epsilon'} \right) \frac{ee'}{cc}$  has a given finite although very small value, which is positive or negative according as  $ee'$  is positive or negative; and  $r_0 = \frac{r}{r + \frac{uu}{cc} \cdot \frac{r-\rho}{\rho}}$  has also a positive or negative value differing from nothing, since the initial values of  $r$  and  $uu$ , by which  $r_0$  is to be determined, must be considered as *positive measurable quantities* to be determined by experiment.

When  $\frac{\rho}{r_0}$  is positive because both numerator and denominator are positive, all the movements are confined to the distances outside the interval  $\rho r_0$ , and are divisible into *movements at a distance* and *molecular movements* which are separated from each other by the interval  $\rho r_0$ .

But if  $\frac{\rho}{r_0}$  is positive because numerator and denominator are both negative, the movements extend to all possible distances, since the interval  $\rho r_0$  then lies outside all possible distances.

When  $\frac{\rho}{r_0}$  is negative, in which case the interval  $\rho r_0$  lies partly outside and partly within the possible distances, all the movements are confined to the part of the interval  $\rho r_0$  lying within possible distances; and if  $\rho$  is positive and  $r_0$  negative, they are *molecular movements*.

From this it follows, when  $\rho$  and  $r_0$  are positive, that, in the first place, no transition from *movements at a distance* to *molecular movements* takes place; secondly, that  $uu$  always remains less than  $cc$ , if it was smaller at first; and thirdly, that when  $uu$  is less than  $cc$ ,  $r$  and  $r_0$  are (both at once) either greater or less than  $\rho$ .

If we keep merely to experience, some of these relative movements of the two particles may be left entirely out of account, for it is evident that infinitely great relative velocities are never met with in reality; on the contrary,  $\frac{1}{cc} \cdot \frac{dr^2}{dt^2}$  is almost always to be considered a very small fraction.

This limitation, derived from the nature of things, is also tacitly assumed when  $V = \frac{ee'}{r} \left( \frac{1}{cc} \cdot \frac{dr^2}{dt^2} - 1 \right)$  is taken as the *potential*, since this must be  $=0$  for an infinitely great value of  $r$ . For if  $\frac{dr^2}{dt^2}$  were infinitely great, the expression  $\frac{ee'}{r} \left( \frac{1}{cc} \cdot \frac{dr^2}{dt^2} - 1 \right)$  might have a value differing from nothing even for infinitely great values of  $r$ .

But if the value of  $\frac{dr^2}{dt^2}$  is never infinitely great, there must be a finite value which  $\frac{dr^2}{dt^2}$  never exceeds. We may assume  $cc$  as such a value.

Presupposing this limitation of the relative velocities,  $r_0$  is always positive; and for every value of  $r_0$  there exists only a single, always continuous series of corresponding values of  $r$  and

$\frac{dr^2}{dt^2}$ ; and

when  $\rho$  is positive and  $r_0$  is  $> \rho$ ,

the corresponding values of  $r$  and  $\frac{dr^2}{dt^2}$  extend from  $r=r_0$  to  $r=\infty$  and from  $\frac{dr^2}{dt^2}=0$  to  $\frac{dr^2}{dt^2}=\frac{\rho}{r_0}$ . The movements are in this case *movements at a distance*.

If  $\rho$  is positive and  $r_0 < \rho$ , or if  $\rho$  is negative,

the corresponding values extend from  $r=r_0$  to  $r=0$ , and from  $\frac{dr^2}{dt^2}=0$  to  $\frac{dr^2}{dt^2}=cc$ . In the first case, when  $\rho$  is positive and  $r_0 < \rho$ , and likewise in the second case, when  $\rho$  is negative and  $r_0 < \rho$ , the movements are *molecular movements*; but if, in the second case,  $r_0$  is  $> \rho$ , the movements are partly *movements at a distance* and partly *molecular movements*.

Hence, with the above limitation of the movements, we obtain for two particles  $e, e'$ , moving solely in consequence of their reciprocal action, if there is *no motion of rotation of the particles about each other* in space, the following equation of motion, namely,

$$\frac{uu}{cc} = \frac{r-r_0}{r-\rho} \cdot \frac{\rho}{r_0},$$

in which  $u$  is put  $= \frac{dr}{dt}$ , and where  $\rho$  has a value that is given by the particles  $e, e'$ , their masses  $\epsilon, \epsilon'$ , and the constant  $c$ , and  $r_0$  denotes a constant to be determined, according to this very equation, by the initial value of  $r$  (which must be positive and not equal to  $\rho$ , but otherwise may be any thing whatever) and the initial value of  $uu$  (which must be positive and less than  $cc$ , but otherwise may be any thing whatever).

#### 10. *Two states of aggregation of a system of two particles of the same kind.*

For two like particles the value of  $\rho$  is positive. And since, moreover, for every value of  $r$  the relative velocity  $u$  may have two equal but opposite values, the value of  $r$  may, in accordance with the above equation  $\frac{uu}{cc} = \frac{r-r_0}{r-\rho} \cdot \frac{\rho}{r_0}$ ,

either *at first* decrease from  $r=\infty$  to  $r=r_0$ ,  $u$  at the same time

increasing from  $u = -c\sqrt{\frac{\rho}{r_0}}$  to  $u=0$ , and *afterwards*



$r$  may increase again from  $r=r_0$  to  $r=\infty$ ,  $u$  at the same time increasing from  $u=0$  to  $u=+c\sqrt{\frac{\rho}{r_0}}$ ;

or  $r$  may at first decrease from  $r=r_0$  to  $r=0$ ,  $u$  at the same time decreasing from  $u=0$  to  $u=-c$ , and then afterwards  $r$  may increase from  $r=0$  to  $r=r_0$ ,  $u$  at the same time decreasing from  $u=-c$  to  $u=0$ .

It is easily seen that in the *first* case the motion is *not a reverting one*; for, after the distance  $r$  has diminished from any given value to  $r_0$ , it increases again without limit; that is, it never decreases again. In the *latter* case, on the other hand, the motion is *reverting*, for the distance  $r$  alternately diminishes from  $r_0$  to 0 and increases again from 0 to  $r_0$ .

There seems indeed to be a sudden change in the value of the velocity  $u$  from  $-c$  to  $+c$  at the moment when  $r=0$ ; but no sudden change occurs in reality; for, when  $r$  vanishes,  $-c$  denotes the same velocity as  $+c$  does when  $r$  is increasing again from zero.

These two cases of motion are moreover distinguished from each other by the fact that *no transition* takes place from one to the other; for, according to the above equation, such a transition, in the case of the interval  $\rho r_0$  or  $r_0 \rho$  could only occur by  $u$  taking imaginary values.

Now upon this separateness of the two kinds of motion a distinction may be founded between *two states of aggregation of a system of two similar particles*—that is, between a state of aggregation in which the particles can only move at a distance from each other, and a state of aggregation in which they can take part only in molecular movements. A transition from the one state of aggregation to the other cannot take place so long as both particles move in consequence of their reciprocal action only.

It only remains to be noted further, that it has been here presupposed that the two particles, considered in space, possessed no motion except in the direction of  $r$ ; but in the next section the opposite case will be considered.

#### 11. Motion of two Electrical Particles which move in space with different velocities, in directions at right angles to the straight line joining them.

Let  $\alpha$  denote the difference of the two velocities which two electrical particles  $e$  and  $e'$ , at a distance  $r$  from each other, possess in space in a direction perpendicular to the straight line  $r$  which joins them; then  $\frac{\alpha\alpha}{r}$  denotes the part of the relative acceleration  $\frac{du}{dt}$  which depends upon  $\alpha$ .

If we deduct this part  $\frac{\alpha\alpha}{r}$  from the total acceleration  $\frac{du}{dt}$ , the difference  $\left(\frac{du}{dt} - \frac{\alpha\alpha}{r}\right)$  expresses that part of the relative acceleration of the two particles which results from the forces exerted by them upon each other. According to section 9 this latter part was  $= \left(\frac{1}{\epsilon} + \frac{1}{\epsilon'}\right) \frac{dV}{dr}$ ; and hence we obtain the following equation,

$$\frac{du}{dt} - \frac{\alpha\alpha}{r} = \left(\frac{1}{\epsilon} + \frac{1}{\epsilon'}\right) \frac{dV}{dr}.$$

Multiplying this equation by  $u dt = dr$ , we get

$$u du - \alpha\alpha \frac{dr}{r} = \left(\frac{1}{\epsilon} + \frac{1}{\epsilon'}\right) \cdot \frac{dV}{dr} dr;$$

and hence, by integrating from the instant at which  $u=0$ , the value of  $r$  corresponding to this instant being denoted by  $r_0$ ,

$$\left(\frac{1}{\epsilon} + \frac{1}{\epsilon'}\right) (V - V_0) = \frac{1}{2} uu - \int_{r_0}^r \frac{\alpha\alpha}{r} dr,$$

in which  $V = \frac{ee'}{r} \left(\frac{uu}{cc} - 1\right)$  and  $V_0 = -\frac{ee'}{r_0}$ , but where, in order to perform the last integration,  $\alpha\alpha$  must be represented as a function of  $r$ .

Now  $r \cdot \alpha dt$  is the element of surface described by the line connecting the two repelling or attracting particles while they move about each other for the element of time  $dt$ ; and for equal elements of time  $dt$  this superficial element retains always the same value, whence  $r\alpha dt = r_0\alpha_0 dt$ . Introducing the resulting value

$$\alpha\alpha = r_0 r_0 \alpha_0 \alpha_0 \cdot \frac{1}{rr}$$

in the last member of the above equation, and carrying out the integration, we obtain the following equation,

$$2 \left(\frac{1}{\epsilon} + \frac{1}{\epsilon'}\right) \frac{ee'}{cc} \left(\frac{r-r_0}{rr_0} + \frac{1}{r} \cdot \frac{uu}{cc}\right) = \frac{uu}{cc} + \frac{\alpha_0\alpha_0}{cc} \cdot \frac{r_0 r_0 - rr}{rr};$$

from which, by putting  $2 \left(\frac{1}{\epsilon} + \frac{1}{\epsilon'}\right) \frac{ee'}{cc} = \rho$ , the equation of motion

$$\frac{uu}{cc} = \frac{r-r_0}{r-\rho} \left(\frac{\rho}{r_0} + \frac{r+r_0}{r} \cdot \frac{\alpha_0\alpha_0}{cc}\right)$$

is obtained. Putting this value of  $\frac{uu}{cc}$  into the equation

$$V = \frac{ee'}{r} \left( \frac{uu}{cc} - 1 \right),$$

we get

$$V = \frac{ee'}{r} \left( \frac{r-r_0}{r-\rho} \left( \frac{\rho}{r_0} + \frac{r+r_0}{r} \cdot \frac{\alpha_0 \alpha_0}{cc} \right) - 1 \right),$$

$$\frac{dV}{dr} = \frac{ee'}{r} \cdot \frac{r_0 - \rho}{(r-\rho)^2} - \frac{ee'}{(r-\rho)^2} \left( 1 - \left( 3 - 2 \frac{\rho}{r} \right) \frac{r_0 r_0}{rr} \right) \frac{\alpha_0 \alpha_0}{cc}.$$

12.

According to the last section, there exists an equation between the relative velocity  $u$  and the relative distance  $r$  of two particles moving anyhow *in space* under the action of their reciprocal forces, namely the equation

$$\frac{uu}{cc} = \frac{r-r_0}{r-\rho} \left( \frac{\rho}{r_0} + \frac{r+r_0}{r} \frac{\alpha_0 \alpha_0}{cc} \right),$$

in which  $\rho$  denotes a constant that is *positive* for two *similar* particles, and *negative* for two *dissimilar* particles.

Now from this there follow results relative to the free motions of two particles *in space*, which move, under the influence of their own reciprocal action, with unequal velocities in a direction perpendicular to the straight line joining them, quite similar to those arrived at in relation to the motions considered in section 10 *in the direction of the straight line*  $r$ . There results, in fact, in this case also, a distinction between two states of aggregation for two *similar* particles—namely, a state of aggregation in which the two particles move in such a way as to return periodically into the same position relatively to each other, and a state of aggregation in which the two particles move so as to become always more and more distant from each other and never return to the same position. No transition from one state of aggregation to the other takes place so long as the two particles move only under the influence of their own reciprocal forces.

13.

A rotation of the two particles about each other implies the existence of a certain *attracting force* if the two particles are to remain at a constant distance from each other during this rotation; and this attracting force required for the rotation increases, for the same distance, according to the square of velocity of rotation. According to this, one would expect that, for two *similar* electrical particles at a distance  $r_0 < \rho$  (at which they attract each



other), there would be always a *certain velocity of rotation*  $\alpha_0$  for which the attracting force required by the rotation should be equal to the attracting force resulting from the reciprocal action of the two particles, so that the two particles rotating about each other would remain, for this velocity of rotation, at the same distance  $r_0$ . This, however, is not the case, since the attracting force resulting from the reciprocal action of the two particles depends not only upon the distance  $r_0$ , but also upon the velocity of rotation  $\alpha_0$ , and increases with the latter in such a manner that it always remains greater than the attracting force required by the rotation, so that with any such rotation there is always involved a mutual approach of the two particles.

It follows indeed easily that, in the case of two *similar* particles  $e$  and  $e'$ , when  $\rho$  has a *positive* value and  $r=r_0$ , and consequently  $u=0$ , there is no value of  $\alpha_0$  for which  $\frac{du}{dt}=0$ , as must be the case if the two particles are to remain at an invariable distance  $r_0$ . For when  $r=r_0$ , it results from the equation at the end of section 11 that

$$\frac{dV}{dr} = \frac{ee'}{r_0(r_0-\rho)} \left(1 + 2 \frac{\alpha_0 \alpha_0}{cc}\right);$$

and from this it further follows, since

$$\frac{du}{dt} - \frac{\alpha\alpha}{r} = \left(\frac{1}{\epsilon} + \frac{1}{\epsilon'}\right) \frac{dV}{dr} = \frac{\rho cc}{2ee'} \cdot \frac{dV}{dr},$$

that

$$\frac{du}{dt} = \frac{1}{2} \frac{cc}{r_0-\rho} \left(\frac{\rho}{r_0} + 2 \frac{\alpha_0 \alpha_0}{cc}\right),$$

whence  $\frac{du}{dt}$  can be equal to nothing only when

$$\alpha_0 \alpha_0 = -\frac{1}{2} \frac{\rho}{r_0} cc,$$

which for a *positive* value of  $\rho$  (that is, when  $e$  and  $e'$  are of the *same kind*) is impossible.

It follows further that, in the case of two *similar* particles, if  $r=r_0$ ,  $\frac{du}{dt}$  is either *positive* or *negative*, according as  $r_0 > \rho$  or  $r_0 < \rho$ .

Consequently the two particles separate always to a greater and greater distance from each other when  $r=r_0 > \rho$ , and approach always nearer to each other when  $r=r_0 < \rho$ , whatever value  $\alpha_0$  may have.

#### 14. On the Time of Oscillation of an Electrical Atomic Pair.

Two *similar* electrical particles at a distance  $r_0 < \rho$  from each other (at which their relative velocity  $=0$ ) do not remain at this

distance, but approach each other from  $r=r_0$  to  $r=0$  with a velocity which increases from  $u=0$  to  $u=\sqrt{\left(cc+\frac{r_0r_0\alpha_0\alpha_0}{\rho}\cdot\frac{1}{r}\right)}$ —that is to say, becomes infinite, if the velocity of rotation  $\alpha_0$  differed from nothing for the instant at which  $r=r_0$ . From this it follows that the interval of time  $\Theta$  in which the two particles approach each other from the distance  $r=r_0$  to  $r=0$  has a finite value. The fact that for the instant at which  $r$  becomes equal to 0 the value of the relative velocity of the two particles becomes

$$\sqrt{\left(cc+\frac{r_0r_0\alpha_0\alpha_0}{\rho}\cdot\frac{1}{r}\right)}=\pm\infty,$$

signifies here only that this relative velocity is to be henceforward taken as a velocity of separation  $=+\infty$ , whereas it was, up to this point, a velocity of approach  $=-\infty$ . This being premised, it easily follows that, in a second equal interval of time  $\Theta$ , the two particles will separate from each other again from the distance  $r=0$  to the distance  $r=r_0$ . The interval of time  $2\Theta$ , in which the two particles approach each other with increasing velocity from the distance  $r=r_0$  to  $r=0$  and then separate again from the distance  $r=0$  to  $r=r_0$ , may be called the *time of oscillation* of the *atomic pair* formed of the two electrical particles.

There still remains the problem of *determining the time of oscillation  $2\Theta$  of such an atomic pair.*

This time of oscillation can be readily deduced from the equation

$$\frac{uu}{cc}=\frac{r-r_0}{r-\rho}\left(\frac{\rho}{r_0}+\frac{r_0+r}{r}\cdot\frac{\alpha_0\alpha_0}{cc}\right),$$

if it be assumed that therein  $r_0$  is not greater than  $\rho$ .

For if we *first* consider the limiting case in which  $r_0=\rho$ , it follows from the above equation that

$$uu=cc+\alpha_0\alpha_0+\rho\alpha_0\alpha_0\cdot\frac{1}{r};$$

and hence, putting  $u=\frac{dr}{dt}$ ,

$$dt=-dr\sqrt{\frac{r}{\rho\alpha_0\alpha_0+(cc+\alpha_0\alpha_0)r}}.$$

From this we obtain, by integration,

$$\Theta=-\int_{\rho}^0 dr\sqrt{\frac{r}{\rho\alpha_0\alpha_0+(cc+\alpha_0\alpha_0)r}}.$$

Accordingly we get:—

$$\Theta = \frac{\rho}{cc + \alpha_0 \alpha_0} \sqrt{(cc + 2\alpha_0 \alpha_0)} \\ - \frac{\rho \alpha_0 \alpha_0}{(cc + \alpha_0 \alpha_0)^{\frac{3}{2}}} \log \left( \sqrt{\left(1 + \frac{cc}{\alpha_0 \alpha_0}\right)} + \sqrt{\left(2 + \frac{cc}{\alpha_0 \alpha_0}\right)} \right);$$

or, for small values of  $\frac{\alpha_0}{c}$ ,

$$\Theta = \frac{\rho}{c} \left( 1 - \frac{\alpha_0 \alpha_0}{cc} \log \frac{2c}{\alpha_0} \right).$$

If we *next* confine ourselves to the consideration of *small oscillations* (that is to say, those for which  $\frac{r_0}{\rho}$  is very small), it results from the above equation, when  $r_0$  and  $r$  are taken as vanishingly small compared with  $\rho$ , that

$$uu = \frac{r_0 r_0 \alpha_0 \alpha_0}{\rho} \cdot \frac{1}{r} + cc - \left( \frac{cc}{r_0} + \frac{\alpha_0 \alpha_0}{\rho} \right) r;$$

whence, putting  $u = \frac{dr}{dt}$ ,

$$cdt = -dr \sqrt{\frac{r}{\frac{r_0 r_0 \alpha_0 \alpha_0}{\rho cc} + r - \left( \frac{1}{r_0} + \frac{\alpha_0 \alpha_0}{\rho cc} \right) rr}},$$

which leads to an elliptic integral. For vanishing values of  $\frac{\alpha_0}{c}$ , we obtain

$$cdt = -dr \sqrt{\frac{1}{1 - \frac{r}{r_0}}};$$

whence there comes, by integration,

$$\Theta = -\frac{1}{c} \int_{r_0}^0 \frac{dr}{\sqrt{\left(1 - \frac{r}{r_0}\right)}} = \frac{2r_0}{c}.$$

When, as has been assumed,  $r$  is  $< \rho$ ,  $r_0$  may be called the amplitude of oscillation; and it follows that, for small values of  $\frac{\alpha_0}{c}$  and for small amplitudes of oscillation, the time of oscillation  $2\Theta$  of an electrical atomic pair is proportional to the amplitude of oscillation  $r_0$ . But the factor with which  $r_0$  must be multiplied in order to give  $2\Theta$ , though a constant  $= \frac{4}{c}$  for small amplitudes, diminishes for greater amplitudes, and becomes  $= \frac{2}{c}$  for the amplitude  $r = \rho$ .



If we put  $c = 439450 \cdot 10^6 \frac{\text{millimetre}}{\text{second}}$ , it follows from this last determination that the value of  $\rho$  must lie approximately between  $\frac{1}{4000}$  and  $\frac{1}{8000}$  of a millimetre in order that these oscillations may be equal in rapidity to those of light.

The difference of the electrical particles  $e$ ,  $e'$  and of their masses  $\epsilon$ ,  $\epsilon'$  in the case of small values of  $\frac{\alpha_0}{c}$  and small amplitudes, does not affect the oscillations at all; and in the case of greater amplitudes it affects them only so far as the value of  $\rho$  depends upon it.

### 15. Applicability to Chemical Atomic Groups.

The distinction between two or more states of aggregation of bodies, according as they consist of simple atoms, or of atomic pairs, or of groups of more than two atoms, has acquired great importance in relation to *chemistry*. Now one, and now another state of aggregation occurs; and in many chemical processes a transition takes place from one to another; but the intermediate states which occur in the case of such transition cannot exist permanently, and those states of aggregation are consequently completely separate from each other as *permanent states*.

Now it is obvious that the *permanence* of some atomic conditions, which are distinguished as special states of aggregation, and the *want of permanence* in all other atomic conditions, may have its cause in the laws of the reciprocal action of atoms—that is, in the difference between the forces exerted upon each other by atoms according to the different relations in which they may stand towards each other. The cause of the permanence of some atomic states and of the want of this permanence in others has not hitherto been recognized in the laws of the reciprocal action of atoms; and it would doubtless be difficult to succeed in discovering this cause in such laws of reciprocal action as it has hitherto been attempted to establish and to assume for ponderable atoms.

The question consequently presents itself, whether the cause of the permanence of certain atomic states may not perhaps be found in such laws of mutual action as have here been established and assumed for electrical particles. Hence the movements of two electrical particles under the influence of the reciprocal action assigned to them, which have been followed out in the preceding sections, are of interest in connexion with this point also, since in them a cause has been really discovered upon which the existence of such permanent states of aggregation may be founded. And in relation to this it is to be specially

observed that the same forces as those which determine the *states of aggregation of electricity* formed by simple atoms and by atomic pairs, may possibly also determine similar *states of aggregation of ponderable bodies*. For in the general distribution of electricity it must be assumed that an atom of electricity adheres to each ponderable atom. But if atoms of electricity adhere firmly to ponderable atoms, nothing will be altered in the relations of the electrical atoms except the *masses* which have to be moved by the forces acting on the electrical atoms. But in the preceding developments the *masses* are left undetermined, and are simply denoted by  $\epsilon$  and  $\epsilon'$ ; while the electrical particles themselves, to which the masses  $\epsilon$  and  $\epsilon'$  belong, are determined, without a knowledge of the values  $\epsilon$  and  $\epsilon'$ , by the measurable quantities  $e$  and  $e'$ . If now we take the values of  $\epsilon$  and  $\epsilon'$  so great as to include the masses of the ponderable atoms adhering to the electrical atoms, all the results that have been arrived at in reference first of all to *electrical atoms* merely, may also be applied to the ponderable atoms combined with the electrical atoms.

#### 16. On the state of Aggregation and Oscillation of two dissimilar Electrical Particles.

In the case of two *dissimilar* electrical particles, the same equations hold good as in the case of two similar particles, namely those of section 11; that is to say,

$$\frac{uu}{cc} = \frac{r-r_0}{r-\rho} \left( \frac{\rho}{r_0} + \frac{r+r_0}{r} \cdot \frac{\alpha_0 \alpha_0}{cc} \right),$$

$$V = \frac{ee'}{r} \left[ \frac{r-r_0}{r-\rho} \left( \frac{\rho}{r_0} + \frac{r+r_0}{r} \cdot \frac{\alpha_0 \alpha_0}{cc} \right) - 1 \right],$$

$$\frac{dV}{dr} = \frac{ee'}{(r-\rho)^2} \left[ \frac{r_0-\rho}{r_0} - \left( 1 - \frac{3r-2\rho}{r^3} \cdot r_0 r_0 \right) \frac{\alpha_0 \alpha_0}{cc} \right],$$

where  $\rho = 2 \left( \frac{1}{\epsilon} + \frac{1}{\epsilon'} \right) \frac{ee}{cc}$ ; the only difference is, that when the particles are *dissimilar*  $\rho$  has a *negative* value, because the product  $ee'$  is *negative*. Besides these equations we have also  $ar = \alpha_0 r_0$  (since only such motions are considered as are made by two electrical particles under the action of their own reciprocal action), whence there follows, lastly, the equation

$$\frac{du}{dt} = \frac{1}{2} \frac{\rho cc}{ee'} \cdot \frac{dV}{dr} + \frac{r_0 r_0 \alpha_0 \alpha_0}{r_3}.$$

Hence it results that, as in the case of two similar electrical par-

ticles, when  $r=r_0$ ,

$$\frac{dV}{dr} = \frac{ee'}{r_0(r_0-\rho)} \left(1 + 2 \frac{\alpha_0 \alpha_0}{cc}\right),$$

$$\frac{du}{dt} = \frac{1}{2} \frac{cc}{r_0-\rho} \left(\frac{\rho}{r_0} + 2 \frac{\alpha_0 \alpha_0}{cc}\right);$$

and that, when also  $\alpha_0 = \sqrt{-\frac{\rho cc}{2r_0}}$  (which has now a real value,

since  $-\rho = -2\left(\frac{1}{\epsilon} + \frac{1}{\epsilon'}\right)\frac{ee'}{cc}$  is positive for dissimilar particles),

$\frac{du}{dt} = 0$ ; according to which, when  $r=r_0$  and  $\alpha_0 = \sqrt{-\frac{\rho cc}{2r_0}}$ , the two particles in their rotation about each other *remain always at the same distance* ( $=r_0$ ) *apart*, a case which with two *similar* particles cannot occur at all.

It follows, however, further from the equation

$$\frac{uu}{cc} = \frac{r-r_0}{r-\rho} \left(\frac{\rho}{r_0} + \frac{r+r_0}{r} \frac{\alpha_0 \alpha_0}{cc}\right),$$

or, when we put  $n$  for the constant value  $-\frac{r_0 r_0 \alpha_0 \alpha_0}{\rho cc}$ , from the following equation,

$$-\frac{r-\rho}{\rho} \cdot \frac{uu}{cc} = \left(\frac{r}{r_0} - 1\right) \cdot \left[n\left(\frac{1}{r_0} + \frac{1}{r}\right) - 1\right],$$

that besides the value  $r=r_0$ , for which  $u=0$  is given, there is in general also another value of  $r$ , namely  $\frac{nr_0}{r_0-n}$ , for which likewise  $u=0$ .

These two values of  $r$ , however, for which  $u=0$ , differ from each other sometimes to a greater and sometimes to a smaller extent, according to the value of  $n$ ; and when  $n = \frac{r_0}{2}$  (that is to

say, when  $\alpha_0 = \sqrt{-\frac{\rho cc}{2r_0}}$ ), they coincide completely; and it is

only when the two values of  $r$  for which  $u=0$  coincide thus that the previously mentioned case occurs, for which we have at the

same time  $u=0$  and  $\frac{du}{dt}=0$ ; and consequently the two particles, while revolving round each other, remain at the same distance.

In all other cases in which the velocity  $u=0$  (as, for example, when  $r=2n-x$ , where  $x < n$ ) there is also a second value of  $r$ —in this case  $2n + \frac{nx}{n-x}$ ,—for which also the velocity  $u=0$ .  $\frac{du}{dt}$



has then a positive value for  $r=2n-x$ , but diminishes and becomes equal to nothing between  $r=2n-x$  and  $r=2n+\frac{nx}{n-x}$ ; so that, for  $r=2n+\frac{nx}{n-x}$ ,  $\frac{du}{dt}$  has a negative value. It is evident from this that repulsion of the two particles takes place from  $r=2n-x$  as far as the value of  $r$  for which  $\frac{du}{dt}=0$ , and attraction from this point as far as  $r=2n+\frac{nx}{n-x}$ , and consequently that the two particles must always remain in *oscillatory motion relatively to each other within the indicated limits*.

### 17. On Ampère's Molecular Currents.

The molecular state of aggregation of two dissimilar electrical particles that has just been described, namely that in which the distance of the two particles alternately increases and diminishes between exactly defined limits and the path in which one particle moves about the other becomes a circular orbit at the two limits, is deserving of closer consideration, especially in those cases in which it is admissible to regard one of the particles as being at rest and the other particle as moving in a circle about the first.

The relation between the particles in respect of their participation in the motion depends upon the ratio of their masses  $\epsilon$  and  $\epsilon'$ ; and, according to section 15, the values of  $\epsilon$  and  $\epsilon'$  must include the masses of the ponderable atoms adhering to the electrical atoms. Let  $e$  be the positive electrical particle, and let the negative particle be equal and opposite to it, and let it therefore be denoted by  $-e$  (instead of by  $e'$ ). Now let a ponderable atom adhere to the latter only, whereby its mass is so much increased that the mass of the positive particle becomes negligible in comparison. The particle  $-e$  may then be regarded as being at rest, and the particle  $+e$  alone as being in motion around the particle  $-e$ .

The two dissimilar particles, when in the molecular state of aggregation that has been described, consequently represent an *Ampèrian molecular current*; for it can be shown that they correspond completely to the assumptions which Ampère made in relation to the *molecular currents*.

In order to show this, let us develop the expression for the force which the *moving particle*  $e$  exerts upon any given element of a current. Let  $ds'$  denote the length of the given element of current,  $+e'ds'$  the positive, and  $-e'ds'$  the negative electricity which it contains; and, lastly, let  $u'$  denote the velocity of the positive particle  $+e'ds'$ , and  $-u'$  the velocity of the negative

particle  $-e'ds'$ . Also, let  $r$  denote the distance of the element of current from the particle  $e$ ,  $u$  the velocity of the particle  $e$ ,  $x, y, z$  the coordinates of the particle  $e$ ,  $x', y', z'$  the coordinates of the element of current,  $\Theta$  and  $\Theta'$  the angles which the directions of  $u$  and  $u'$  make with  $r$ , and  $\epsilon$  the angle between the directions of  $u$  and  $u'$ .

Next, let the general expression for the repelling force of two electrical particles  $e$  and  $e'$  at the distance  $r$ , namely

$$\frac{ee'}{rr} \left( 1 - \frac{1}{cc} \cdot \frac{dr^2}{dt^2} + \frac{2r}{cc} \frac{ddr}{dt^2} \right),$$

be transformed as follows (see Beer, *Einleitung in die Elektrostatik, die Lehre vom Magnetismus und die Electrodynamik*, S. 251). First, let the equation

$$rr = (x-x')^2 + (y-y')^2 + (z-z')^2$$

be differentiated with respect to the time  $t$ ; we then get

$$r \frac{dr}{dt} = (x-x') \left( \frac{dx}{dt} - \frac{dx'}{dt} \right) + (y-y') \left( \frac{dy}{dt} - \frac{dy'}{dt} \right) + (z-z') \left( \frac{dz}{dt} - \frac{dz'}{dt} \right),$$

or also

$$r \frac{dr}{dt} = r(u \cos \Theta - u' \cos \Theta').$$

By a second differentiation we get

$$\begin{aligned} \frac{dr^2}{dt^2} + r \frac{ddr}{dt^2} &= \left( \frac{dx}{dt} - \frac{dx'}{dt} \right)^2 + \left( \frac{dy}{dt} - \frac{dy'}{dt} \right)^2 + \left( \frac{dz}{dt} - \frac{dz'}{dt} \right)^2 \\ &+ (x-x') \left( \frac{ddx}{dt^2} - \frac{ddx'}{dt^2} \right) + (y-y') \left( \frac{ddy}{dt^2} - \frac{ddy'}{dt^2} \right) \\ &+ (z-z') \left( \frac{ddz}{dt^2} - \frac{ddz'}{dt^2} \right), \end{aligned}$$

wherein

$$\left( \frac{dx}{dt} - \frac{dx'}{dt} \right)^2 + \left( \frac{dy}{dt} - \frac{dy'}{dt} \right)^2 + \left( \frac{dz}{dt} - \frac{dz'}{dt} \right)^2 = u^2 + u'^2 - 2uu' \cos \epsilon.$$

If now the acceleration of the one particle, whose components are  $\frac{ddx}{dt^2}, \frac{ddy}{dt^2}, \frac{ddz}{dt^2}$ , be denoted by  $N$ , and the angle which its direction makes with  $r$  by  $\nu$ , and in like manner the acceleration of the other particle, whose components are  $\frac{ddx'}{dt^2}, \frac{ddy'}{dt^2}, \frac{ddz'}{dt^2}$ , by  $N'$ , and the angle which its direction makes with  $r$  by  $\nu'$ , we obtain

$$\begin{aligned} \frac{x-x'}{r} \left( \frac{ddx}{dt^2} - \frac{ddx'}{dt^2} \right) + \frac{y-y'}{r} \left( \frac{ddy}{dt^2} - \frac{ddy'}{dt^2} \right) + \frac{z-z'}{r} \left( \frac{ddz}{dt^2} - \frac{ddz'}{dt^2} \right) \\ = N \cos \nu - N' \cos \nu'. \end{aligned}$$

The substitution of these values gives

$$2 \frac{dr^2}{dt^2} + 2r \frac{ddr}{dt^2} = 2(u^2 + u'^2 - 2uu' \cos \epsilon) + 2r(N \cos \nu - N' \cos \nu'),$$

$$3 \frac{dr^2}{dt^2} = 3(u \cos \Theta - u' \cos \Theta)^2.$$

The second equation subtracted from the first gives

$$- \frac{dr^2}{dt^2} + 2r \frac{ddr}{dt^2} = 2(u^2 + u'^2 - 2uu' \cos \epsilon) - 3(u \cos \Theta - u' \cos \Theta)^2$$

$$+ 2r(N \cos \nu - N' \cos \nu'),$$

whence the general expression for the repelling force of two electrical particles  $e$  and  $e'$  at the distance  $r$ , namely

$$\frac{ee}{rr} \left( 1 - \frac{1}{cc} \frac{dr^2}{dt^2} + \frac{2d}{cc} \frac{dr}{dt^2} \right),$$

is obtained in the following transformed shape,

$$= \frac{ee'}{ccrr} [cc + 2(u^2 + u'^2 - 2uu' \cos \epsilon) - 3(u \cos \Theta - u' \cos \Theta)^2$$

$$+ 2r(N \cos \nu - N' \cos \nu')].$$

By substituting for the particle  $e'$  the positive electricity in the given element of current, namely  $+e'ds$ , this expression gives the repelling force

$$\frac{ee'ds'}{ccrr} [cc + 2(u^2 + u'^2 - 2uu' \cos \epsilon) - 3(u \cos \Theta - u' \cos \Theta')^2$$

$$+ 2r(N \cos \nu - N' \cos \nu')];$$

but by putting for the particle  $e'$  the negative electricity in the given element of current, namely,  $-e'ds'$ , we obtain the repelling force

$$\frac{ee'ds'}{ccrr} [-cc - 2(u^2 + u'^2 + 2uu' \cos \epsilon) + 3(u \cos \Theta + u' \cos \Theta)^2$$

$$- 2r(N \cos \nu + N' \cos \nu')],$$

since in this case  $\epsilon + \pi$ ,  $\Theta' + \pi$ , and  $\nu' + \pi$  take the place of  $\epsilon$ ,  $\Theta'$ , and  $\nu'$ ; and these therefore give together the total repelling force between the moving particle  $e$  and the whole element of current, namely

$$\frac{4ee'ds'}{ccrr} (3uu' \cos \Theta \cos \Theta' - 2uu' \cos \epsilon - rN' \cos \nu').$$

The repelling force between the stationary particle  $-e$  and the whole element of current, on the other hand, if  $r$  denotes the distance of the stationary particle  $-e$  from the given element of



current, is

$$+ \frac{4ee'ds'}{ccrr} \cdot rN' \cos \nu',$$

since in this case  $u=0$ . But the difference between the value given to  $r$  here and that assigned to it previously (namely the distance from the particle  $+e$ , in motion about the particle  $-e$ , to the given element of current), may be regarded as a negligible fraction of  $r$ , so that we get, for the repelling force exerted by the moving particle  $+e$  and stationary particle  $-e$  together upon the element of current, the expression

$$\frac{4ee'ds'}{ccrr} (3 \cos \Theta \cos \Theta' - 2 \cos \epsilon) \cdot uu'.$$

If we were to put in place of the moving electrical particle  $+e$  a second element of current, the positive electricity of which, moving with the velocity  $+\frac{1}{2}u$ , was denoted by  $+eds$ , and whose negative electricity, moving with the velocity  $-\frac{1}{2}u$ , was denoted by  $-eds$ , we should obtain for the mutual repelling force of the two elements of current the value

$$= \frac{4eds \cdot e'ds'}{ccrr} (3 \cos \Theta \cos \Theta' - 2 \cos \epsilon') \cdot uu';$$

that is to say, the same expression as before, if the electrical particle previously denoted by  $+e$  (and moving with the velocity  $u$ ) were taken as equal to the positive electricity contained in the second element of current, namely  $+eds$  (moving with the velocity  $\frac{1}{2}u$ ).

It follows from this that the rotatory motion of the electrical particle  $+e$  about the stationary particle  $-e$  replaces a circular double current, if the positive electricity contained in the latter is equal to  $+e$  and moves in its circular orbit with half the velocity of the aforesaid electrical particle  $+e$ , and if also the negative electricity contained in the current is equal to  $-e$  and moves with the same velocity as the positive electricity but in the opposite direction.

Hence it appears that an electrical particle  $+e$  moving in a circle about the electrical particle  $-e$  exerts upon all galvanic currents the same effects as those assumed by Ampère in the case of his molecular currents.

The molecular currents assumed by Ampère, however, differ essentially from all other galvanic currents in this respect, that, according to Ampère's assumption, they *continue* without electromotive force; whereas all other galvanic currents, in accordance with Ohm's law, are proportional to the electromotive force, and *cease* when the electromotive force vanishes. But it is evi-

dent that the electrical particle  $+e$ , spoken of above, must of itself, without electromotive force, continue indefinitely its rotatory motion about the particle  $-e$ , and therefore must correspond entirely with the molecular currents assumed by Ampère in this respect also.

We accordingly obtain in this way, as a deduction from the *laws of the molecular state of aggregation of two dissimilar electrical particles*, developed in the preceding section, a simple construction for the molecular currents assumed by Ampère without proof that their existence was possible.

18. *Movements of two dissimilar Particles in Space under the Action of an Electrical Segregating Force* (Scheidungskraft).

If  $\pi + v$  denotes the angle which the direction of the electrical segregating force makes with  $r$ , and  $a$  denotes the magnitude of the relative acceleration of the two particles depending upon the segregating force,  $-a \cdot \cos v$  and  $a \cdot \sin v$  are the components of  $a$ ,—the *former* expressing the part of the relative acceleration  $\frac{du}{dt}$  which is dependent on the segregating force, and the latter

the part of  $\frac{d\alpha}{dt}$  which depends on the same force, where  $\alpha$  is the difference of the velocities of the two particles in a direction perpendicular to  $r$ . It is presupposed that the direction of the segregating force lies in the plane in which the two particles rotate about each other.

If now the *first* component, namely  $-a \cdot \cos v$ , as the part of  $\frac{du}{dt}$  which depends upon the *segregating force*, and also  $\frac{\alpha\alpha}{r}$ , as the part of  $\frac{du}{dt}$  which depends upon the *velocity*  $\alpha$ , be deducted from the total acceleration  $\frac{du}{dt}$ , the difference

$$\left(\frac{du}{dt} + a \cdot \cos v - \frac{\alpha\alpha}{r}\right)$$

denotes the part of the relative acceleration which results from the force which the two particles  $e$  and  $e'$  exert upon each other, namely

$$\left(\frac{1}{\epsilon} + \frac{1}{\epsilon'}\right) \frac{dV}{dr} = \frac{\rho}{2} \frac{cc}{ee'} \cdot \frac{dV}{dr};$$

and hence the following equation is obtained:—

$$\frac{du}{dt} + a \cdot \cos v - \frac{\alpha\alpha}{r} = \frac{\rho}{2} \frac{cc}{ee'} \cdot \frac{dV}{dr}$$

If we deduct the *last* component, namely  $a \cdot \sin v$ , as the part

of the acceleration  $\frac{d\alpha}{dt}$  which depends upon the *segregating force*, from the total value  $\frac{d\alpha}{dt}$ , the difference  $\left(\frac{d\alpha}{dt} - a \sin v\right)$  gives that part of the total acceleration  $\frac{d\alpha}{dt}$  which results from *the existing motion under the sole influence of the forces exerted upon each other by the two particles*. But, under the sole influence of the attracting or repelling forces exerted upon each other by the two particles, the element of surface  $\alpha r dt$ , described in a given element of time  $dt$ , would have a constant value, or we should have  $\alpha \frac{dr}{dt} + r \frac{d\alpha}{dt} = 0$ ; hence the resulting part of the acceleration  $\frac{d\alpha}{dt}$  becomes

$$-\frac{\alpha}{r} \frac{dr}{dt}.$$

By equating this part with the above difference, we get the equation

$$\frac{d\alpha}{dt} - a \sin v = -\frac{\alpha}{r} \frac{dr}{dt}.$$

Besides these, we have, as is self-evident, a third equation,

$$dv = \frac{\alpha dt}{r}.$$

Accordingly, for the four variable magnitudes  $r, u, \alpha, v$ , there are the following three equations:—

$$a \cos v - \frac{\alpha \alpha}{r} = \frac{\rho c c}{2 e e'} \cdot \frac{dV}{dr} - \frac{du}{dt}, \quad . \quad . \quad . \quad (1)$$

$$a \sin v - \frac{\alpha dr}{r dt} = \frac{d\alpha}{dt}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$dv = \frac{\alpha dt}{r}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Multiplying equation (1) by  $dr = u dt$ , and equation (2) by  $r dv = \alpha dt$ , we obtain

$$a \cos v \cdot dr - \frac{\alpha \alpha dr}{r} = \frac{\rho c c}{2 e e'} \cdot \frac{dV}{dr} dr - u du, \quad . \quad . \quad (4)$$

$$a r \sin v \cdot dv - \frac{\alpha \alpha dr}{r} = \alpha d\alpha. \quad . \quad . \quad . \quad . \quad . \quad (5)$$

The difference of these two equations gives

$$a \cdot d(r \cos v) = \frac{\rho c c}{2 e e'} \cdot \frac{dV}{dr} dr - \alpha d\alpha - u du. \quad . \quad . \quad (6)$$



We also get from (2) and (3),

$$-2ar^3 \cdot d(\cos v) = d(\alpha^2 r^2). \quad . \quad . \quad . \quad (7)$$

The integration of the differential equation (6) gives, after multiplying by 2 and putting  $V = \frac{ee'}{r} \left( \frac{uu}{cc} - 1 \right)$ ,

$$2ar \cos v = \frac{\rho cc}{r} \left( \frac{uu}{cc} - 1 \right) - \alpha\alpha - uu + \text{const.}; \quad . \quad . \quad (8)$$

and from this, since  $r=r_0$ ,  $\alpha=\alpha_0$ , and  $\cos v=-1$  when  $u=0$ , comes

$$-2ar_0 = -\frac{\rho cc}{r_0} - \alpha_0\alpha_0 + \text{const.} \quad . \quad . \quad . \quad (9)$$

Equation (9), subtracted from equation (8), gives

$$2ar \cos v + 2ar_0 = \left( \frac{\rho}{r} - 1 \right) uu + \rho cc \left( \frac{1}{r_0} - \frac{1}{r} \right) - \alpha\alpha + \alpha_0\alpha_0. \quad (10)$$

By integrating the differential equation (7) we obtain, after dividing by  $r^3$ ,

$$-2a \cos v = \frac{\alpha\alpha}{r} + 3 \int \frac{\alpha\alpha dr}{rr},$$

or, multiplying by  $r$ ,

$$-2ar \cos v = \alpha\alpha + 3r \int \frac{\alpha\alpha dr}{rr}, \quad . \quad . \quad . \quad (11)$$

and hence, for the sum of (10) and (11),

$$2ar_0 = \left( \frac{\rho}{r} - 1 \right) uu + \rho cc \left( \frac{1}{r_0} - \frac{1}{r} \right) + \alpha_0\alpha_0 + 3r \int \frac{\alpha\alpha dr}{rr},$$

and therefore

$$uu = \frac{1}{r-\rho} \left( \rho cc \left( \frac{r}{r_0} - 1 \right) + r\alpha_0\alpha_0 + 3rr \int \frac{\alpha\alpha dr}{rr} - 2ar_0 r \right). \quad (12)$$

From equation (3) there follows further, since  $dr=udt$ ,

$$dv = \frac{\alpha}{u} \frac{dr}{r}; \quad . \quad . \quad . \quad . \quad . \quad (13)$$

and since, by equation (7),

$$d(\cos v) = -\frac{d(\alpha^2 r^2)}{2ar^3},$$

and by equation (11),

$$\cos v = -\frac{1}{2\alpha} \left( \frac{\alpha\alpha}{r} + 3 \int \frac{\alpha^2 dr}{r^2} \right),$$

we get, by substituting these values in the identical equation

$$dv = -\frac{d(\cos v)}{\sqrt{(1 - \cos v^2)}},$$

according to equation (13),

$$\frac{\alpha}{u} \frac{dr}{r} = \frac{\frac{d(\alpha^2 r^2)}{2ar^3}}{\sqrt{\left(1 - \frac{1}{4a^2} \left(\frac{\alpha^2}{r} + 3 \int \frac{\alpha^2 dr}{r^2}\right)^2\right)}};$$

and from this and equation (12),

$$\begin{aligned} uu &= \left(\frac{\alpha r^2 dr}{d(\alpha^2 r^2)}\right)^2 \cdot \left(4a^2 - \left(\frac{\alpha^2}{r} + 3 \int \frac{\alpha^2 dr}{r^2}\right)^2\right) \\ &= \frac{1}{r-\rho} \left(\frac{r-r_0}{r_0} \rho c^2 + r(\alpha_0^2 - 2ar_0) + \rho r^2 \int \frac{\alpha^2 dr}{r^2}\right), \quad (14) \end{aligned}$$

or the following equation for the two variables  $r$  and  $\alpha$  :—

$$\begin{aligned} 4a^2 &= \left(\frac{\alpha^2}{r} + 3 \int \frac{\alpha^2 dr}{r^2}\right)^2 \\ &+ \frac{4}{r-\rho} \left(\frac{d(\alpha r)}{dr}\right)^2 \cdot \left(\frac{r-r_0}{r_0} \cdot \frac{\rho c^2}{r^2} + \frac{\alpha_0^2 - 2ar_0}{r} + 3 \int \frac{\alpha^2 dr}{r^2}\right)^*. \quad (15) \end{aligned}$$

If we now confine ourselves to small values of  $a$ , for which  $\alpha r$  is not, indeed, constant, as it is for  $a=0$ , according to section 11, but for which it differs only little from a constant value  $\alpha_0 r_0 = n$ , we may put

$$\alpha r = n(1 + \epsilon), \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

where  $\epsilon$  has always a very small value. It then follows from this that

$$\frac{\alpha^2}{r} = (1 + 2\epsilon) \frac{n^2}{r^3}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

$$\frac{d(\alpha r)}{dr} = n \frac{d\epsilon}{dr}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

Further, by (11) and (17),

$$\int \frac{d\epsilon}{r^3} = - \frac{a}{n^2} \cos v,$$

\* If the segregating force  $a$  vanish,  $\alpha r$  must, according to section 11, assume a constant value. But for a constant value of  $\alpha r$  and for  $a=0$ , equation (15) reduces itself to

$$0 = \frac{\alpha^2}{r} + 3 \int \frac{\alpha^2 dr}{r^2};$$

and this, divided by the constant value  $\alpha^2 r^2$ , gives the identical equation

$$0 = \frac{1}{r^3} + 3 \int \frac{dr}{r^4},$$

in accordance with section 11.

or

$$d\epsilon = \frac{a}{n^2} r^3 \sin v dv; \quad . \quad . \quad . \quad . \quad . \quad (19)$$

from (18) and (19),

$$\frac{d(\alpha r)}{dr} = \frac{a}{n} r^3 \sin v \cdot \frac{dv}{dr}; \quad . \quad . \quad . \quad . \quad . \quad (20)$$

and from (17) and (19),

$$\frac{\alpha^2}{r} = \frac{n^2}{r^3} + \frac{2a}{r^3} \int r^3 \sin v dv. \quad . \quad . \quad . \quad . \quad . \quad (21)$$

If we now substitute the values of  $\frac{d(\alpha r)}{dr}$  and  $\frac{\alpha^2}{r}$  given by (20) and (21) in the following equation resulting from (11) and (15), namely

$$\alpha^2 \sin v^2 = \frac{1}{r-\rho} \cdot \left( \frac{d(\alpha r)}{dr} \right)^2 \cdot \left( \frac{r-r_0}{r_0} \cdot \frac{\rho c^2}{r^2} + \frac{\alpha_0^2 - 2ar_0}{r} - \frac{\alpha^2}{r} - 2a \cos v \right), \quad (22)$$

we obtain, by again putting for  $n$  its value  $\alpha_0 r_0$ , the following equation between  $r$  and  $v$ , namely

$$\begin{aligned} \frac{\alpha_0^2 r_0^2}{r^4 c^2} \cdot \frac{dr^2}{dv^2} &= \frac{r-r_0}{r-\rho} \left( \frac{\rho}{r_0} + \frac{r+r_0}{r} \cdot \frac{\alpha_0^2}{c^2} \right) \\ &\quad - \frac{2a}{(r-\rho)c^2} \left( r_0 r + \frac{3}{r} \int r^2 \cos v dr \right)^*. \quad . \quad (23) \end{aligned}$$

By differentiating this equation, after multiplying it by  $r(r-\rho)$ , we obtain

$$\begin{aligned} \frac{d}{dr} \left( (r-\rho) \frac{\alpha_0^2 r_0^2}{r^3 c^2} \cdot \frac{dr^2}{dv^2} \right) &= \frac{\rho r}{r_0} + (r+r_0) \left( \frac{\alpha_0^2}{c^2} + (r-r_0) \left( \frac{\rho}{r_0} + \frac{\alpha_0^2}{c^2} \right) \right. \\ &\quad \left. - \frac{2a}{c^2} (2r_0 r + 3r^2 \cos v) \right). \end{aligned}$$

\* From the above equation, since  $\frac{r}{\alpha} u$  may be substituted for  $\frac{dr}{dv}$ , we obtain

$$\frac{\alpha_0^2 r_0^2}{\alpha^2 r^2} \cdot \frac{uu}{cc} = \frac{r-r_0}{r-\rho} \left( \frac{\rho}{r_0} + \frac{r+r_0}{r} \cdot \frac{\alpha_0^2}{c^2} \right) - \frac{2a}{(r-\rho)c^2} \left( r_0 r + \frac{3}{r} \int r^2 \cos v dr \right),$$

which, when the segregating force  $a$  vanishes, and therefore, according to section 11,  $\alpha r = \alpha_0 r_0$ , passes over into the equation

$$\frac{uu}{cc} = \frac{r-r_0}{r-\rho} \left( \frac{\rho}{r_0} + \frac{r+r_0}{r} \cdot \frac{\alpha_0^2}{c^2} \right) -$$

that is to say, into the same equation that was arrived at already for this case in section 11.



If we here put, to consider a special case,

$$\rho = -\frac{2r_0}{cc}(\alpha_0^2 + ar_0)$$

(that is to say, the case in which, for  $a=0$ , the two particles remain, according to section 16, at the same distance during their rotation), we obtain

$$\frac{d}{dr} \left( (r-\rho) \frac{\alpha_0^2 r_0^2}{r^3 c^2} \cdot \frac{dr^2}{dv^2} \right) = -\frac{2(r-r_0)}{cc}(\alpha_0^2 + ar_0) - \frac{6ar}{cc}(r_0 + r \cos v),$$

which becomes  $=0$ , *first*, when  $u=0$  and consequently  $r=r_0$ ,  $\alpha=\alpha_0$ , and  $\cos v = -1$ , and, *secondly*, when

$$r_0 - r = \frac{3ar(r_0 + r \cos v)}{\alpha_0^2 + ar_0},$$

a case which occurs for small values of  $a$ , if  $\cos v = +1$  and so  $r=r_0 - \frac{6ar_0^2}{\alpha_0^2}$  approximately.

Hence it follows that, just as, according to section 16, one of two dissimilar electrical particles, for which  $\rho = -2r_0 \frac{\alpha_0 \alpha_0}{cc}$ , could move round the other in a circular orbit when *not acted on by segregating force*, so also when two dissimilar electrical particles, for which  $\rho = -2r_0 \left( \frac{\alpha_0 \alpha_0}{cc} + ar_0 \right)$ , *are acted on by a segregating force* ( $=a$ ), one of them can revolve about the other in a closed orbit, though the orbit is not circular. The distance between the particles varies, in fact, according as the moving particle lies before or behind the central particle considered relatively to the direction of the segregating force, being in the latter case  $=r_0$ , and in the former case  $=r_0 - 6 \frac{r_0 r_0}{\alpha_0 \alpha_0} a$ .

Such an eccentric position of the one particle in the plane of the orbit described (under the influence of a segregating force) by the other particle about this one, may be compared to the separation of electric fluids at rest under the influence of a similar segregating force; but the remarkable difference presents itself that the separation takes place in opposite directions in the two cases.

It follows from this, that in all conductors that have been charged in the usual way under the influence of a force of electrical segregation, the electricity cannot be contained only in the state of aggregation corresponding to Ampère's molecular currents, since in that case the resulting segregation would take

place in the opposite direction to that which actually does occur. But even if all the electricity in such a conductor existed in the form of Ampèrian molecular currents before the action of the segregating force began, there must have been amongst these molecular currents some which could not persist under the action of the segregating force (one particle continuing to revolve in a closed orbit round the other), and were accordingly broken up, the two particles separating more and more from each other until they arrived at the boundary of the conductor. Under the influence of the force of segregation, the positive and negative particles of the broken molecular currents could remain at rest only when distributed in a particular way on the surface of the conductor; but when the force of segregation ceased to act, they would enter into motion again until they had again united themselves two by two into Ampèrian molecular currents.

### 19. *Electrical Currents in Conductors.*

If all the electricity in conductors were contained in them (before a segregating force began to act) in the state of aggregation corresponding to Ampèrian molecular currents, which, however, were incapable of persisting under the action of a segregating force, but were broken up, so that the two dissimilar electrical particles, which were revolving about each other, separated further and further from each other, until their paths finally approached asymptotically the direction of the segregating force, dissimilar electrical particles derived from different molecular currents would encounter each other before they could reach the boundaries of the conductor, and would form with each other new molecular currents. These newly formed molecular currents would then in their turn be broken up, and the particles constituting them would again separate further and further from each other in paths asymptotically approaching the direction of the segregating force, and so on.

Thus there would arise a current of electricity in the conductor in the direction of the segregating force. If the conductor had the shape of a uniform ring, and if the segregating force had the same intensity in every separate element of length of the ring and acted in the direction of the element, a constant circular current would be produced in the ring, and the laws of motion of electrical particles under the action of a force of electrical segregation, developed in the previous section, would form the basis of the theory of these constant electrical currents in closed conductors.

Here it is evident that, during the existence of this current, *work* would be done by each particle, since it moves forward under the action of the segregating force in the direction of this

force. And since all the other forces which act upon such a particle in a conductor must together balance each other, this work will make its appearance as an equivalent increase of the *vis viva* of the particle; whence it follows that the *vis viva* of all the Ampèrian molecular currents contained in the conductor must, while the current traverses the conductor, increase; that is to say, the square of the velocity with which the particles in the Ampèrian molecular current revolve about one another must increase proportionally to the force of segregation (*electromotive force*), and proportionally to the distance through which this force acts in its own direction (or to the *strength of the current*). If the ratio of the *electromotive force* to the *strength of the current* be called *resistance*, we may say instead of the above that the *vis viva* of all the molecular currents contained in the conductor increases, during the passage of the current, proportionally to the *resistance*, and proportionally to the *square of the strength of the current*.

This increase of *kinetic energy* of the electrical particles contained in a conductor while a current traverses it, follows therefore as a necessary consequence of the action of the electromotive force upon the particles, while these particles, as the result of the current, move onward in the direction of this force.

This theoretical conclusion receives, not indeed a direct, but an indirect confirmation from experiment, inasmuch as an increase of *thermal energy* is observed in the conductor while a current traverses it. And this *observed* increase of the *thermal energy* in the conductor is equal to the *calculated* increase of the *kinetic energy* of the electrical particles in the Ampèrian molecular currents of the conductor.

Now the *thermal energy* of a body is a *kinetic energy* resulting from movements in the *interior of the body*, which are therefore inaccessible to direct observation. In like manner, the *kinetic energy* belonging to the electrical particles in the Ampèrian molecular currents in a conductor is a kinetic energy which results from movements taking place in the *interior of the conductor*, and therefore inaccessible to direct observation.

But notwithstanding this agreement, the *thermal energy* of a body and this kinetic energy of the electrical particles in the Ampèrian currents contained in the same body might possibly be altogether different as to their essential nature. For it is possible that the *thermal energy* might be energy resulting from the motion of quite other particles than those of electricity, and the motion of these other particles might be of quite a different kind from those of the particles in Ampèrian currents.

In order to explain the identity of the increase of the energy of the Ampèrian molecular currents, as determined above, with



the increase of thermal energy found by observation, it would then be absolutely necessary, *according to the principle of the conservation of energy*, that a *transference* should take place of the kinetic energy of the electrical particles in the Ampèrian currents to the other particles whose motion constitutes heat. And indeed it would be needful that *all* the kinetic energy produced by the current in the electrical particles of the Ampèrian currents should be *completely* transferred to these other particles at each instant.

But apart from the consideration that it is impossible to conceive how such a *complete* transference could take place, it is self-evident that any even partial transference of the kinetic energy of Ampèrian molecular currents to other particles is contradictory of the *permanence* which belongs to the essential nature of Ampèrian currents. If such a transference of kinetic energy from electrical particles in molecular currents to other particles were really to occur, it would simply prove that the molecular currents formed by these particles were not *Ampèrian molecular currents*, since they would not possess the permanence wherein the essence of Ampèrian molecular currents consists.

Hence it follows as a consequence that, if in conductors all the electrical particles exist in the state of aggregation corresponding to Ampèrian molecular currents, the observed increase in the *thermal energy* of a conductor, during the passage of a current through it, must result *immediately* from the increase of the *kinetic energy* of the electrical particles constituting the Ampèrian currents; that is to say, the *thermal energy* imparted to the conductor by the current must be *kinetic energy* due to motions in the interior of the conductor, and must in fact consist in an increase in the strength of the Ampèrian currents formed by the electrical particles in the conductor.

Reference may also be made, in connexion with the identity of *thermal energy and the kinetic energy of Ampèrian molecular currents*, to what is said respecting "the Transformation of the work of the current into Heat," in the 10th volume of the *Abhandlungen der K. Ges. d. Wiss. zu Göttingen* (1862), in the 33rd section of the memoir entitled "*Zur Galvanometrie.*"

## 20. On Thermomagnetism.

The following remark readily connects itself with the hypothesis of the previous section, that the electricity in conductors exists in the state of aggregation corresponding to Ampèrian molecular currents—and with the consequent identity of the *thermal energy* of the conductor and the *kinetic energy* of the Ampèrian currents in the conductor—namely, that *equality of temperature* in two conductors must depend upon certain rela-

tions between the strength and character of the Ampèrian currents in the two conductors, but that, along with the relation needed for this equality of temperature, the following difference may exist between the currents of the two conductors, namely:— that *greater masses* of electricity may move with *smaller velocity* in the Ampèrian currents of the one conductor, and *smaller masses* of electricity with *greater velocity* in those of the other conductor.

Let now a ring be conceived, formed of two such dissimilar conductors, through which a constant current passes, so that in the same time an equal quantity of electricity passes through every section of the ring; then it is evident that equal quantities of electricity must also traverse the two sections which bound the *first layer of the second conductor*. But the electricity which traverses the first section comes from the *first conductor*, in the molecular currents of which large masses of electricity move with small velocity. Hence, in consequence of this smaller velocity, this electricity which penetrates into the *first layer of the second conductor* possesses less *vis viva*. The electricity which passes through the second section comes from the above-mentioned first layer of the second conductor itself, where a smaller mass of electricity moves in the Ampèrian currents with a greater velocity, and therefore it possesses, in consequence of this greater velocity, a greater *vis viva*. It follows from this, that, as a consequence of the current, this *first layer of the second conductor* gives up more *vis viva* to the following layer of the second conductor than it receives from the last layer of the first conductor. Consequently a diminution takes place in the kinetic energy of the Ampèrian currents of this layer, or, in other words, a *diminution of the thermal energy or temperature*.

The opposite condition is found on considering the two sections which bound the *first layer of the first conductor*. The electricity which passes through the first section into this layer comes out of the end of the *second conductor* with a greater velocity; and that which passes out of this layer through the second section, leaves this section with a smaller velocity; whence it follows that, as a consequence of the current, the *first layer of the first conductor* gives up less *vis viva* to the following layer of the same conductor than it receives from the last layer of the second conductor; and thus an increase takes place in the kinetic energy of the Ampèrian currents of this layer, or, in other words, an *increase of the thermal energy or temperature*.

It will be seen that a foundation is here presented for the doctrine of *thermomagnetism*, and in particular for Peltier's fundamental experiment, although it would lead us too far to pursue it further here.

It may suffice merely to add here a similar remark in relation to Seebeck's fundamental thermomagnetic experiment. In a body which possesses the same temperature in all its parts, the heat is supposed to be in a state of *mobile equilibrium*; or we speak, with Fourier, of a *reciprocal radiation* of the particles of the body, by virtue of which each particle parts with just as much heat to the surrounding particles as it receives from them. Now, if heat consists in Ampèrian molecular currents, which, however, are broken up by the positive and negative particles separating from each other until they encounter other particles, with which they form new molecular currents, equilibrium of temperature must consist in this, that the *vis viva* of the electrical particles which leave any part of the body is equal to the *vis viva* of the electrical particles which enter this part of the body.

Let us now consider the surface of contact of two conductors which differ from each other only by greater masses of electricity moving with smaller velocity in the Ampèrian currents of one, and smaller masses moving with greater velocity in those of the other. Then, when both the conductors are at the same temperature, the *vis viva* of the electrical particles which pass from the first conductor into the second must be equal to the *vis viva* of the electrical particles that pass from the second conductor into the first; but the *mass* of the electrical particles which pass from the first conductor into the second would be greater than the *mass* of the electrical particles which pass out of the second conductor into the first. But from this (if the electricity which passes over is always positive, while the negative electricity remains behind in the conductor, to the particles of which it adheres) there would result a *difference of electrical charge on the two sides of the surface of contact*; that is to say, there would result an *electromotive force* at this surface of contact; for the electromotive force of a surface of contact is a force whereby a difference of electrical charge is produced at the two sides of the surface of contact.

If now the two conductors are of such a nature that this difference of charge at the two sides of their surface of contact is not always the same, but is *greater or less according to variations of temperature*, there would follow the production of a current in a ring formed of these two conductors, if different temperatures were to exist at the two surfaces of contact of the conductors.

## 21. Helmholtz on the contradiction between the Law of Electrical Force and the Law of the Conservation of Force.

In his memoir, "Ueber die Bewegungsgleichungen der Elektrizität für ruhende leitende Körper," in the *Journal für die reine und angewandte Mathematik* (vol. lxxii. pp. 7 and 8),



Helmholtz deduces from the law of electrical force the equation of motion of two electrical particles for motions in the direction of the distance  $r$  of the two particles, namely

$$\frac{1}{cc} \cdot \frac{dr^2}{dt^2} = \frac{C - \frac{ee'}{r}}{\frac{1}{2}mcc - \frac{ee'}{r}},$$

or, putting  $C = \frac{ee'}{r_0}$  and  $\frac{2ee'}{mcc} = \rho$ , the equation

$$\frac{1}{cc} \frac{dr^2}{dt^2} = \frac{r - r_0}{r - \rho} \cdot \frac{\rho}{r_0};$$

that is to say, the same equation as was arrived at in section 9.

If  $\frac{ee'}{r} > \frac{1}{2}mcc > C$ —that is, if  $\frac{\rho}{r} > 1 > \frac{\rho}{r_0}$ , we have  $\frac{dr^2}{dt^2}$  positive and greater than  $cc$ , and  $\frac{dr}{dt}$  is therefore real. If the latter is also positive,  $r$  will increase until  $\frac{ee'}{r} = \frac{1}{2}mcc$ , that is till  $r = \rho$ , and then  $\frac{dr}{dt}$  becomes infinitely great.

The same will happen if, to begin with,  $C > \frac{1}{2}mcc > \frac{ee'}{r}$ ; that is, if  $\frac{\rho}{r_0} > 1 > \frac{\rho}{r}$ , and  $\frac{dr}{dt}$  is negative.

These consequences are, according to Helmholtz, in contradiction with the law of the conservation of force.

Now it may be remarked hereupon, in the first place, that two electrical particles are here assumed which begin to move with a *finite* velocity certainly, but one which is greater than the velocity  $c$ —greater, that is, than  $439450 \cdot 10^6 \frac{\text{millimetre}}{\text{second}}$ .

The case of two bodies moving relatively to each other with such a velocity is nowhere recognizable in nature. In all practical cases we are accustomed rather to treat  $\frac{1}{cc} \frac{dr^2}{dt^2}$  as a very small fraction; and this deserves notice.

For, according to Helmholtz (*loc. cit.* p. 7), a law is in contradiction with the law of the *conservation of force* if two particles, moving in accordance with it and beginning with a *finite* velocity, attain, within a finite distance of each other, *infinite* vis viva, and so are able to do an infinitely great amount of work.

The principle seems to be here announced that, according to the law of the conservation of force, two particles cannot, under any circumstances, possess infinite *vis viva*.

For the above assertion may evidently be inverted, and we may say a law is in contradiction with the law of the conservation of force, if two particles, moving in accordance with it and beginning with *infinite* velocity, attain, at a finite distance from each other, finite *vis viva*, and thus suffer an infinitely great diminution of the work which they are able to perform.

The two particles must therefore always retain an infinite velocity; for if they have not lost it in any finite distance, however great, they would, in accordance with the nature of potential, never lose it even at greater distances. But bodies which always move relatively to each other with an infinite velocity are excluded from the region of our inquiries.

But if two particles never possess more than finite *vis viva*, there must be a finite limiting value of *vis viva* which they never exceed. It is consequently possible that this limiting value for two electrical particles  $e$  and  $e'$  may be  $= \frac{ee'}{\rho}$ ; that is, that the square of the velocity, with which the two particles move relatively to each other, may not exceed  $cc$ .

The contradiction urged by Helmholtz would, according to this, lie not in the law, but in his assumption, according to which the two particles began to move with a velocity the square of which, namely  $\frac{dr^2}{dt^2}$ , was  $> cc$ .

If such a determination of the limiting value of *vis viva* is assumed in connexion with the *law of the conservation of force* according to Helmholtz, it may equally well be assumed in connexion with the *fundamental law of electrical action* (see section 4); that is, the *work* denoted there by  $U$ , as well as the *vis viva* denoted by  $x$  (in the law  $U + x = \frac{ee'}{\rho}$ ), may both be regarded as being *by their nature positive quantities*.

In the second place, it may be remarked that, though the two electrical particles do attain infinite *vis viva* at a finite distance from each other, this finite distance is  $\rho = \frac{2ee'}{cc} \left( \frac{1}{\epsilon} + \frac{1}{\epsilon'} \right)$ , which, according to our measures, is an *undefinable small distance*, for the same reasons that the electrical masses  $\epsilon$  and  $\epsilon'$  are themselves undefinable according to our measures. This distance was consequently denominated in section 9 a molecular distance.

The *theory of molecular motions* requires in any case a special development, which as yet is wanting throughout. But as long as such a theory remains excluded from mechanical investigations, any doubts as to *physical admissibility* in relation to *molecular motions* are without foundation.

It may be remarked, in the third place, that the same objection, namely that two particles, which begin with finite velocity, attain infinite *vis viva* at a finite distance from each other, applies also to the law of gravitation, if it is assumed that the masses of ponderable particles are *concentrated in points*. But if this objection is got rid of, in the case of the law of gravitation, by assuming that the masses even of the smallest particles *occupy space*, we must make the same assumption in relation to electrical particles, in which case it results that only a vanishingly small part of such a particle arrives at a given instant at the distance  $\rho$ ; another vanishingly small part, which arrived at the distance  $\rho$  at the previous instant, will have exchanged its infinitely great velocity of approach for an infinitely great velocity of separation. But if these vanishing parts of the smallest particles are solidly connected together, there cannot be any question of such infinite velocities at all.

Even cosmical masses may begin their movements under physically admissible conditions, and, by continuing to move according to the law of gravitation, may come into physically inadmissible conditions, which can be avoided only through the cooperation of *molecular forces confined to molecular distances*. The disregard of this cooperation is, strictly speaking, only temporarily allowable, namely so long as the conditions are such that its influence is either nothing or may be regarded as vanishingly small. But just as little as an objection to the law of gravitation is derived from this fact, ought any objection to the fundamental law of electrical action to be derived from the physically inadmissible conditions to which, according to Helmholtz, this law leads, when it is considered that these inadmissible conditions are connected only with certain molecular distances.

## XVI. Notices respecting New Books.

*Theory of Heat.* By J. CLERK MAXWELL, M.A., F.R.S., Professor of Experimental Physics in the University of Cambridge. London: Longmans, Green, and Co. 1871. (Pp. 312.)

THE subject of this work is correctly indicated by its title; it is a treatise on the *Theory of Heat*; its writer's aim having been to state and enforce the general propositions that have been established regarding the nature and effects of heat, rather than to discuss the particular facts which are summed up in those propositions. It must not be supposed, however, that no notice is taken of the experiments which form the basis of our knowledge of Heat; on the contrary, they are described, where necessary, at sufficient length to bring out the principles involved in them; but they are described



without reference to details of manipulation, and in few cases, if in any, is an experiment described for purposes of mere illustration.

The contents of the volume are as follows:—After an introduction there are two chapters on Thermometry and Calorimetry (the registration of temperature and the measurement of Heat), in which these subjects are very fully treated, the latter chapter containing the definitions of Thermal Capacity, Specific Heat, and Latent Heat. Then follows a statement of the elementary principles of Dynamics, and of the principles of the measurement of Internal Forces. From this point begins the exposition of the principles of Thermodynamics. One chapter (ch. 6) contains an account of *isothermal* lines, in the course of which a pretty full account is given of Dr. Andrew's experiments in illustration of the continuity of the gaseous and liquid states; a second (ch. 7), of *adiabatic* lines; a third (ch. 8) is devoted to Sadi Carnot's Heat Engine and its four operations, which are explained by means of the indicator diagram, and lead up to the definitions of a Cycle, of Efficiency, and of the Absolute Scale of Temperature. Then follow seven chapters of applications. The subjects now indicated occupy about two thirds of the work; the remaining part is mainly taken up with the subjects of radiation and conduction, and ends with a most interesting chapter on the molecular theory of the constitution of bodies. In the course of the work there are discussions on several subjects not hitherto mentioned, which deserve notice as being original in treatment, or as not commonly given in elementary books; such are:—the account of Watt's Indicator and its application to Thermodynamics (p. 102...), the discussion of the Intrinsic Energy of a system of bodies (ch. 12), the account of the determination of heights by the barometer (ch. 14), that of the propagation of Waves by Longitudinal Disturbances (ch. 15), of Fourier's Theory of the conduction of Heat (p. 236...), and the chapter (ch. 20) on Capillarity.

The above account will perhaps convey a not altogether inadequate notion of the contents of Mr. Clerk Maxwell's book. If it were compared chapter by chapter with any of the existing elementary manuals, such as Mr. Balfour Stewart's, it would be seen that it treats with brevity what is ordinarily given at full length, and amplifies what is in most cases cut short. The parts on which Mr. Clerk Maxwell mainly dwells are those presenting the greatest difficulty to the learner. This difficulty is inherent, and would remain very great even were the subject treated by a writer who possessed in the highest degree the art of exposition. But whatever the causes, whether choice of subject or mode of treatment, there can hardly be a doubt that the book will severely task the attention of the reader who comes fresh to the subject; and this we fear will greatly limit its usefulness as a book "adapted for the use of Artisans and Students in Public Schools." Considered, however, as addressed to students already well trained in something more than the elements of mathematics, and familiar with the fundamental laws of mechanics, it would be hard to name a better book. To such readers it will prove an excellent introduction to the very difficult science of Ther-

modynamics. They will find in it, written by a master, an admirable account of the existing state of knowledge as to the nature and effects of heat, of the steps by which that knowledge has been acquired, of its bearing on the molecular constitution of matter, and of the numerous points at which the subject of heat touches the general doctrines of mechanics.

*Observations upon the Climate of Uckfield in the Weald of Sussex.* By C. LEESON PRINCE. London: J. and A. Churchill. 1871.

This is an admirable digest of meteorological facts elucidatory of the climate of Sussex, in which the author treats of pressure, temperature, and rainfall—three of its most important elements. One feature of the work, of much usefulness, is a complete and consecutive history of the weather at Uckfield, monthly from January 1843 to December 1870 inclusive. We do not remember a similar feature, except in the case of Howard's 'Climate of London.' There is something more needed than *numerical* results, valuable as they are. The salient features—hot or cold, wet or dry, clear or cloudy, mild with the progression of vegetable life, frost with its retardation—each finds a place in the brief meteorological chronicles of the months. Only one omission in the work strikes us; it is that of the determination of the elements which are necessary for resolving atmospheric pressure into its constituents, the pressure of dry air and the elasticity of aqueous vapour. The elaborate Tables of temperature and rainfall for twenty-eight years show that these elements have been well worked up. The pressure was not observed until 1854; and during the seventeen years to the close of the observations, it would have added to the value of the work had it been accompanied by Tables of the degree of humidity, the pressure of dry air, and the elasticity of aqueous vapour; for then the mutual dependence of each element on the others would have become apparent. As it is, the Tables show this dependence to a certain extent, the pressure being greatest in February, the driest month. The pressure in October (the wettest month) was very nearly the lowest, December being only .003 inch lower.

We strongly recommend the work to all meteorologists; and even the general reader will find much to interest him; in a word, it bears the same relation to meteorology as White's 'Selborne' does to natural history.

## XVII. *Proceedings of Learned Societies.*

### ROYAL SOCIETY.

[Continued from p. 75.]

Nov. 16, 1871.—General Sir Edward Sabine, K.C.B., President, in the Chair.

THE following communications were read:—

“On a Periodic Change of the Elements of the Force of Terrestrial Magnetism discovered by Professor Hornstein.”

Professor Hornstein, of Prague, has communicated to the Imperial

Academy of Sciences of Vienna a paper entitled "On the dependence of the Earth's Magnetism on the Rotation of the Sun."

He shows that the changes of each of the three elements of the force of terrestrial magnetism (declination, inclination, and horizontal force) indicate a period of  $26\frac{1}{3}$  days. The periodic change of declination for Prague (1870) amounts to  $0.705 \sin (x + 190^\circ 20')$ , where  $x = 0^\circ$  at the commencement of 1870, and  $x = 360^\circ$  at the commencement of 1871. For Vienna the range is a little larger. The range of inclination is nearly one-third of that of declination, that of the intensity nearly 24 units of the 4th decimal (the intensity in June 1870 was nearly 2.0485).

Professor Hornstein regards these changes of the earth's magnetism as the effect of the sun's rotation, and by a mean of several determinations finds for the duration of the period 26.33 days. This number may consequently be regarded as the result of the first attempt to determine the synodic period of the sun's rotation by means of the magnetic needle. The resulting true periodic time of the sun's rotation is 24.55 days, almost exactly agreeing with the time of rotation of the sun-spots in the sun's equator deduced from astronomical observations (according to Spörer 24.541 days).

"Corrections to the Computed Lengths of Waves of Light published in the Philosophical Transactions of the year 1868." By George Biddell Airy, C.B., Astronomer Royal.

The author, after adverting to the process by which in a former paper he had attempted the computation of the Lengths of Waves of Light, for the entire series measured in the Solar Spectrum by Kirchhoff, from a limited number of measured wave-lengths, and to the discordances between the results of these computations and the actual measure of numerous wave-lengths to which he subsequently had access, calls attention to his remark that means existed for giving accuracy to the whole. The object of the present paper is so to use these means as to produce a table of corrections applicable through the entire range of Kirchhoff's lines, and actually to apply the corrections to those computed wave-lengths which relate to spectral lines produced by the atmosphere and by many metals.

Adopting as foundation the comparisons with Ångström's and Ditscheiner's measures given in the former paper, and laying these down graphically, the author remarks that in some parts of the spectrum the agreement of the two experimenters is very close, that in some parts they are irreconcilable, and that in one part (where they agree) there is a peculiarity which leads to the supposition that some important change was made in Kirchhoff's adjustments. He then explains the considerations on which he has drawn a correction-curve, whose ordinates are to give the corrections applicable to his former computed numbers. A general table of corrections is then given, and this is followed by tables of the Lengths of the Light-Waves for the air and metals as corrected by the quantities deduced from that general table.

The author remarks that he has not yet succeeded in discovering any relation among the wave-lengths for the various lines given by



any one metal &c. which can suggest any mechanical explanation of their origin.

Nov. 23.—General Sir Edward Sabine, K.C.B., President, followed by Mr. Francis Galton, Vice-President, in the Chair.

The following communication was read:—

“An Experimental Determination of the Velocity of Sound.” By J. E. Stone, M.A., F.R.S., Astronomer Royal at the Cape of Good Hope.

A galvanic current passes from the batteries at the Royal Observatory, Cape Town, at 1 o'clock, and discharges a gun at the Castle, and through relays drops a time-ball at Port Elizabeth. It appeared to the author that a valuable determination of the velocity of sound might be obtained by measuring upon the chronograph of the Observatory the interval between the time of the sound reaching some point near the gun and that of its arrival at the Observatory. As there is only a single wire between the Observatory and Cape Town, some little difficulty was experienced in making the necessary arrangements, without any interference with the 1 o'clock current to Port Elizabeth; but this difficulty was overcome by a plan which the author describes, and which was brought into successful operation on Feb. 27, 1871. The experiments could not have been carried out, on account of the encroachment they would have made on the time of the Observatory staff, had it not been for the assistance of J. Den, Esq., the acting manager of the Cape Telegraph Company, to whom the author is indebted for the preparation of a good earth-connexion near the gun, for permission to Mr. Kirby, a gentleman attached to the telegraph office, to assist in the experiments, and for a general superintendence of the arrangements at Cape Town.

The observed times of hearing the sound were recorded on the chronograph by two observers, situated one (Mr. Kirby) at a distance of 641 feet from the gun, the other (Mr. Mann) at the Observatory, at a distance of 15,449 feet from the gun. The former distance was sufficient to allow the connexion of the main wire to be broken at the telegraph office after the gun had been fired, but before the sound reached the first observer.

As there were no reciprocal signals, a correction was made by calculation for the effect of the wind, its velocity being measured by a set of Robinson's cups. The personal equation, under the circumstances of the observations, was found as follows:—A gun was fired at such a distance from the Observatory as to be heard with about the same degree of distinctness as the time-gun at the Castle. This distance was found to be 1483 feet. The registrations on the chronograph were made by Mr. Kirby at the distance of 162 feet from the gun, and Mr. Mann at the Observatory. For this comparatively small distance, the time of transit calculated from the velocity deduced from the time taken to travel over the larger distance may be deemed exact. The observed time for the smaller difference of distance was found to be too great by  $0^{\text{s}}.09$ , which correction has been applied to all the observations. It depends more on want of sensi-

bility in picking up and recognizing faint sounds than upon mere habit of making contacts. When the observers were interchanged, the observed interval of time appeared still too large, but in this case by 0<sup>s</sup>.02. It is clear that such personal equations are not eliminated by an interchange of observers, nor by return signals.

In the reduction of the equations, the coefficient of elasticity of air under a constant volume (that is to say, the ratio of the increment of pressure for an increment 1° F. of temperature to the pressure at 32° F.) was regarded as an unknown quantity, as well as V, the velocity of sound at 32° F. The reduction of the equations furnished by the observations, which were 38 in number, gave

$$V = 1090.6 \text{ feet per second,} \\ \alpha = 0.0019,$$

Regnault's value of  $\alpha$  being 0.0020.

There appeared to be but little difference between the residual errors as dependent on the motion of the air. The author grouped the residuals into two classes, according to the dampness of the air; but there appeared to be no appreciable difference in the velocity as dependent upon dampness.

#### GEOLOGICAL SOCIETY.

[Continued from p. 76.]

June 21, 1871.—Joseph Prestwich, Esq., F.R.S., President,  
in the Chair.

The following communications were read:—

1. "On some supposed Vegetable Fossils." By William Carruthers, Esq., F.R.S., F.G.S.

In this paper the author desired to record certain examples of objects which had been regarded, erroneously, as vegetable fossils. The specimens to which he specially alluded were as follows:—Supposed fruits on which Geinitz founded the genus *Guilielmites*, namely *Carpolites umbonatus*, Sternb., and *Guilielmites permianus*, Gein., which the author regarded as the result of the presence of fluid or gaseous matter in the rock when in a plastic state; some roundish bodies, which, when occurring in the Stonesfield slate, have been regarded as fossil fruits, but which the author considered to be the ova of reptiles, and of which he described two new forms; and the flat, horny pen of a Cuttlefish from the Purbeck of Dorsetshire, described by the author as *Teudopsis Brodiei*, sp. n.

2. "Notes on the Geology of part of the County of Donegal." By A. H. Green, Esq., F.G.S.

In this paper the author described the geological structure of the country in the neighbourhood of the Errigal Mountain, with the view of demonstrating the occurrence in this district of an interstratification with mica-schist of beds of rock which can hardly be distinguished from granite, the very gradual passage from alterna-

tions of granitic gneiss and mica-schist into granite alone, and the marked traces of bedding and other signs of stratification that appear in the granite, to which the author ascribed a metamorphic origin. He also noticed the marks of ice-action observed by him in this region, and referred especially to some remarkable fluted bosses of quartzite, and to the formation of some small lakes by the scooping action of ice.

3. "Memoranda on the most recent Geological Changes of the Rivers and Plains of Northern India, founded on accurate surveys and the Artesian well-boring at Umballa, to show the practical application of Mr. Login's theory of the abrading and transporting power of water to effect such changes." By T. Login, Esq., F.R.S.E.

The author commenced by referring to the general conditions of the surface of the country under consideration, and to the evidence afforded by it of a great decrease in the amount of rainfall, and a great change in the nature of the rivers. His object was to show that the superficial deposits of the plains of India were formed by the action of mountain-streams, the deposits being irregular transversely, but exhibiting a uniform section longitudinally, in a curve which the author believed to be a true parabola, as indicated by Mr. Tylor. The connexion of this with the author's theory as to the transporting power of water was indicated. The author also showed that the beds of the large Indian rivers are rising rather than being lowered, and pointed out that this was in accordance with his theory.

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### XVIII. *Intelligence and Miscellaneous Articles.*

#### ON THE SPECTRUM OF HYDROGEN AT LOW PRESSURE.

BY G. M. SEABROKE, ESQ.

**D**URING the late summer months I have been comparing the lines given by hydrogen in the spectroscope with the lines of the solar spectrum, for the purpose of ascertaining whether any lines in the sun's chromosphere were due to hydrogen, besides those usually supposed to be due to this element. The observations are as yet incomplete; but as it will be some months before I can again proceed, I therefore produce the results obtained up to the present time. The experiments have been conducted in a room adjoining the Temple Observatory lately erected at Rugby. My mode of proceeding has been briefly as follows:—I use a vacuum-tube containing hydrogen, and connected with a Sprengel's air-pump. The tube is of the ordinary form, having the part between the bulbs, into which the platinum wires pass, about  $\frac{1}{40}$  inch internal diameter. The pressure in the tube varied from 3 to 4 millims. of mercury. Preliminary experiments showed that at this pressure the lines appeared most distinct; but a slight change of pressure near 4 millims. made little alteration in the lines. There is a battery of twelve Smee's to work the coil for passing the spark in the vacuum-tube.



The light from the hydrogen-tube passed through a lens which concentrates it on the slit of the spectroscope. A dispersive power of four prisms of  $60^\circ$  was used, the arrangement of the instrument being such that the ray of light traverses each prism twice. The room is kept perfectly dark, and sunlight is reflected down from the roof by means of a heliostat. At first I tried the usual mode of comparing spectra, viz. by having the hydrogen and solar spectra side by side. I found this answer very well for the bright lines; but the faint ones could not be distinguished by the side of the bright solar spectrum. I therefore placed a very fine platinum wire in the eyepiece of the spectroscope, and brought the lines under examination into coincidence with the wire, and then passed the sunlight in, and found which black line coincided with the wire, or, where there was no coincidence, the position of the wire with respect to the black lines. I have made from ten to twenty observations on each line that I have at present examined. I have every reason to believe that the limit of error is within two divisions of Fraunhofer's scale either way. The Table below gives the positions of the lines I have already compared. These I hope to examine again next year, and also to finish the remainder.

Reference Number.	Position on Kirchhoff's scale.	Relative brightness. 10 = brightest.	Remarks.
1.	694	10	C.
2.	881	...	Limit of a number of close lines towards C.
3.	930	5	Brightest red line, except C.
4.	1014	5	Suspiciously near the chromospheric line near D.
5.	1049	3	{ The positions of these were taken by reference to the mercury lines, and are therefore not so reliable as the others.
6.	1061	4	
7.	1119	3	
8.	1523	4	
9.	1621	4	
10.	1876	4	
11.	1943	3	
12.	1991	6	
13.	2065	3	
14.	2080	10	F.
15.	2235	4	Near here, exact place uncertain.
16.	2361	6	
17.	2428.5	...	Limit of a band towards F.
18.	2540	2	
19.	2605	3	
20.	2670	3	
21.	2767	...	Faint band.

On comparing the above Table with the catalogue of chromospheric lines by Professor Young, published in the Philosophical Magazine of November 1871, I see no sufficient signs of coincidence to lead me to believe that any of the chromospheric lines in his list are due to hydrogen, except the C and F, already well known to be so due. Since I have not yet examined lines further than 2767, the "near G" (2796) and *h* lines, also known to be due to hydrogen, are

not mentioned in the above list. In these experiments a spectroscope with a large dispersive and magnifying power was found to be required in order to identify the lines in the solar spectrum, so that the hydrogen spectrum became so reduced in brightness that, in order to see the fainter lines, the eye required to be kept for some minutes in the dark room, although with a spectroscope of low power the spectrum appeared very bright and full of lines.—*Monthly Notices of the Royal Astronomical Society*, December 8, 1871.

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ON THE DISENGAGEMENT OF HEAT WHEN CAOUTCHOUC IS  
STRETCHED. BY PROF. E. VILLARI.

In a communication made to the Royal Society in 1857, Joule\* describes some investigations on the thermal effects which occur when threads of various substances are stretched or contract. It results from an accurate investigation of this subject that metal wires cool on being stretched, and become heated on contraction—but that threads of vulcanized caoutchouc have quite the opposite comportment, becoming heated when they are stretched, and cooled when they contract. This remarkable fact has since been investigated by other physicists, for instance by Govi†, and recently by Pierre‡, and always with the same result.

In my recent investigations on the elasticity of caoutchouc, I had occasion to repeat Joule's experiment on threads, cords, and strips of caoutchouc of various dimensions, and by means of very simple apparatus. It consisted of a thermopile connected with a galvanometer, and so suspended to silk threads that it could be easily lowered upon the caoutchouc band to be examined, or raised away from it. The strip of caoutchouc was thus horizontal, fastened at one end to a strong table, and at the other to a lever, so that it could be readily stretched and afterwards loosened. Each time the caoutchouc was stretched or loosened, the thermopile was raised, and then placed with its whole weight upon the caoutchouc, so as to determine the change in temperature produced.

From the experiments made with this apparatus, it followed, in accordance with the labours of the above-mentioned physicists, that caoutchouc becomes heated on being stretched, while it cools on contraction. I may mention, as a new fact (for I am not aware that it has been already observed by others), that the absolute value of the increase of temperature on stretching is greater than the decrease of temperature on contraction. This difference is often observed when the increase of temperature on stretching, and the decrease

\* Phil. Mag. vol. xiv. p. 226. [This remarkable fact was first noticed in unvulcanized caoutchouc by John Gough, by simply holding the caoutchouc against the lips and stretching it. See Nicholson's Journal, vol. xiii. (1806) p. 305.—Ed. Pogg. *Annalen*.]

† *Les Mondes*, April 22, 1809.

‡ Ibid. April 8 and May 6, 1869.

on contraction, are measured by the thermopile. Yet, as the thermal indications are mostly restricted to deflections of 8 or 10 degrees of the galvanometer, it will be easily understood that the small differences are concealed and annulled by the unavoidable errors in such observations. The phenomenon is most conclusively demonstrated when the extensions and contractions are repeated several times in succession, in such a manner that the greater increase of temperature on extension may accumulate as against the smaller decrease on the corresponding contraction. And, in fact, when I frequently and successively stretched and loosened two caoutchouc threads, one 6 to 7 millims. and the other 10 millims. in thickness, I observed with the thermopile so strong an increase on stretching, that the deflection of the galvanometer amounted to 60, 70, or even 90°, especially in the experiments with the thicker thread.

I repeated the same experiments on two caoutchouc bands—one a perfectly smooth one, 25 millims. in breadth and 3 millims. in thickness, and one, which was rough from the impression of the fabric on which it had been made, 27 millims. in breadth and 4 millims. in thickness. Both these strips gave the same results as the above-mentioned threads—that is, became strongly heated on frequent and rapid stretching and loosening. These strips were frequently drawn out with the hand, but usually by means of the above-mentioned lever, the motion of which was confined by means of two fixed points. The object of this was to regulate the rapidity of the stretching and contraction, and thus dissipate a doubt I had entertained, that the generally greater velocity on contraction might have some influence on the result. Hence manifold experiments were made with the bands in question. Sometimes the stretching was made more rapidly than the contraction; and sometimes the latter was quicker than the former. In all cases a powerful increase in temperature was observed in the strips under investigation. To give some idea of the heating, I may observe that a hundred rapid stretchings of the band which was 3 millims. in thickness heated it so that the thermopile placed upon it deflected the galvanometer to the extent of 90°. This deflection rapidly lessened. Similar stretchings made with the band which was 4 millims. in thickness heated it still more—so that the pile deflected the galvanometer with a powerful swing to 90°, and kept it so for several minutes; even after 10 or 12 minutes it still deviated from zero. By only ten stretchings the galvanometer deflected 20°. It is clear that the shorter the time in which the experiment is made the greater is the increase of temperature; for then the loss caused by radiation is less. In repeating the experiment, it is essential that the pile, whether it hangs freely or is placed upon the caoutchouc, show no deflection on the galvanometer. As that part of the galvanometer which touches the pile cools sooner than the free part, I have assured myself, by placing the pile on different parts of the strip, that it had everywhere the same temperature as the vicinity.

The explanation of this is found in the consumption of force



which takes place in stretching and in contracting the caoutchouc. We know, in fact, that when the permanent gases in expanding perform work they become cooled, and the more so the more they were heated while in the compressed state—and, further, that the work performed on compressing a gas by a given amount is equal to that which it produces when it expands, if there is no loss. In caoutchouc there is no such reciprocity, owing to the small mobility of the particles and the resistance which they must overcome in order to displace themselves. Part of the force communicated to them is used up in internal work, and during expansion is converted into work; and another part is also used and changed during the time of contraction. Hence it is that the force used in the expansion of the caoutchouc is not entirely reproduced in the contraction. Such a conclusion results from the following observation. In studying the elasticity of caoutchouc, I have observed that on adding given weights this substance lengthens to a greater extent than that by which it shortens when they are removed. I have recently repeated these experiments; and the results are contained in the following Table :—

Caoutchouc thread 6 millims. in thickness. Length while loaded.

Added.		Removed.		Difference of the loads.
Loads.	Lengths.	Permanent loads.	Lengths.	
	millims.		millims.	
1	246·72	1*	254·50	7·78
2	310·42	2	318·10	7·68
3	396·20	3	419·00	22·80
4	476·58	4	493·20	16·62
5	537·90	5	566·40	28·50
6	586·10	6	622·60	36·50
7	618·60			

Each load corresponds to a weight of 640 grammes.

It follows from the last column of this Table, and of others, which for shortness' sake I will not give, that when a given load is removed the thread is less shortened than it had been lengthened by the addition of the load; and the difference is greater the greater the lengthenings have been.

This observation leads to the assumption that the shortenings are smaller than the lengthenings, not on account of diminished elasticity, but because caoutchouc in its contraction does not develope

\* These experiments were made by adding, one after the other, all the stretching loads until the seventh, and then taking away one each time in the same order. No 1 in this column thus corresponds to a single permanent load, while all the others were removed; No. 2 corresponds to two loads when the others were removed, and so with the other numbers.

the same force as in its lengthening, by which part of the force which would be exerted in contraction is consumed in internal work and changed into heat. Such an internal work must of course also take place in stretching, by which caoutchouc becomes heated—by a series of extensions and contractions, because part of the force exerted in extension is changed into heat, and also because part of the mechanical force which would be developed in contraction is also changed into heat. We might perhaps say that caoutchouc is like a pasty mass in which the particles rub against each other, as in the production of heat. However this may be, the explanation previously given of the development of heat finds a brilliant confirmation in experiments which Warburg has recently made in the celebrated laboratory of Magnus. He proves by various experiments that solids, when they give or transmit a tone, become heated, because the original *vis viva* is changed into internal work and into heat, by which the bodies develop more heat than the sound-motion inherent in them or transmitted by them destroy. Of all bodies investigated, caoutchouc is that which becomes most heated, because it most rapidly extinguishes the sound-vibrations. The analogy between Warburg's observations and my own is complete; and the explanations agree completely with the data previously given. We may then draw the conclusion that those bodies which become cooled on stretching, and heated on contraction, must also be heated by a series of repeated rapid extensions, because in such bodies also part of the force consumed must change into internal work and therefore into heat.—Poggendorff's *Annalen*, No. 10, 1871.

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ACTUAL ENERGY.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

At page 92 of Professor Clerk Maxwell's 'Theory of Heat' (which I have no hesitation in characterizing as the best existing elementary treatise on any branch of physical science) I find the following statement:—"We cannot even assert that all energy must be either potential or kinetic, though we may not be able to conceive any other form."

Now I have to remark that this was the very reason which induced me in 1853 to propose the word "actual" for denoting energy that is not potential, rather than any word expressly denoting motion, and which still induces me to prefer the word "actual" to the word "kinetic" for that purpose.

I am, Gentlemen,

Your most obedient Servant,

W. J. MACQUORN RANKINE.

Glasgow, January 23, 1872.

THE  
LONDON, EDINBURGH, AND DUBLIN  
PHILOSOPHICAL MAGAZINE  
AND  
JOURNAL OF SCIENCE.

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[FOURTH SERIES.]

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MARCH 1872.

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XIX. *On the Constitution of Matter.* By M. B. PELL, B.A.,  
*Professor of Mathematics in the University of Sydney, late Fel-  
low of St. John's College, Cambridge\*.*

IN the following paper an attempt is made to account for some of the properties of matter upon mechanical principles. I assume that solid bodies consist of isolated atoms, whose linear magnitudes are so small compared with the distances between them, that the atoms may be supposed incapable of giving or of receiving any energy except that of translation—and that the mutual action between two atoms is some function of their distance, and acts in the line joining their centres of gravity.

Some writers, whose opinions are entitled to much respect, have expressed an entire want of faith in the theory of isolated atoms acting upon one another at a distance; and some even hold that such a state of things is inconceivable. This is one of those half metaphysical questions upon which perhaps no two men would be found to be exactly agreed; but to me it seems no more difficult to conceive that atoms should have been created with the property of acting upon one another at a distance, than it is to conceive that they should have been created, or have in any way come to exist or to have any properties at all. It may be that the bodies of the solar system do not act upon one another directly; but they appear to do so, and we are content to assume, provisionally at all events, that they do really so act.

\* Communicated by the Author. From the Proceedings of the Royal Society of New South Wales, having been read September 6, 1871.

*Phil. Mag.* S. 4. Vol. 43. No. 285. March 1872. M



There should be no difficulty then in making a similar supposition respecting the atoms of which matter is assumed to consist.

If matter does not consist of isolated atoms but is continuous, any inquiry into its real nature would seem as hopeless as a similar investigation respecting time or space. We could not hope to give any explanation of the facts, for instance, that gold is yellow and soft, and expands under the action of heat, except that it consists of little bits, all of which possess those properties. It will be time to confess that we are reduced to such a method of accounting for the phenomena of nature when every other has been found to fail.

I have endeavoured to assume as little as possible respecting the mutual action of two atoms, except that it must be such a function of the distance as to satisfy the most obvious properties of matter. Leaving out of consideration for the moment all theories except that of gravitation, let us consider what are the facts. Let us consider the mutual action between two atoms or molecules, or particles, or whatever they may be, of a substance such as mercury, which is capable under ordinary circumstances of existing in the solid, the liquid, or the gaseous state. As mercury has weight, we can hardly doubt that at a sufficient distance two particles of that substance attract one another. At some less distance they repel one another, whatever be the cause; for the vapour of mercury, whether *in vacuo* or when mixed with the air, tends to diffuse itself. At a still less distance, in the liquid state, the particles cohere slightly, or attract one another, appearing to be in a relative position of unstable equilibrium. At a slightly diminished distance, the mercury becomes solid and the attractive force considerable. The solid mercury resists further compression, so that the action again becomes repulsive.

During the last century, Boscovich propounded a theory of alternate attractions and repulsions; but I am not aware of the exact nature of his investigations or speculations, never having had an opportunity of consulting his works. They do not appear to have borne fruit, or to have been received with much favour. It is hardly correct, however, to apply the word theory to these attractions and repulsions; we should rather say that they are obvious facts, requiring some theory for their explanation. One theory may be stated thus: the action between two particles is some function of their distance  $x$ , which for considerable distances is sensibly equal to  $\frac{\mu}{x^2}$ , but for very small distances changes sign several times, becoming finally large and negative, or repulsive. Let us consider whether there is any other tenable hypothesis.

The dynamical theory of gases, due chiefly to the labours of

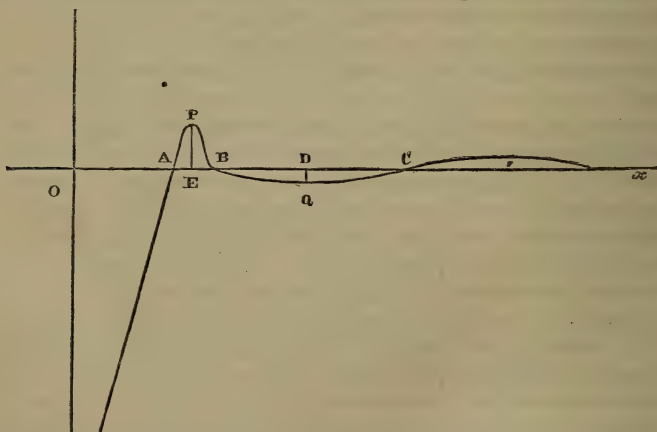
Clausius and Maxwell, helps us somewhat. This theory may be regarded as established, and as forming the most important addition which has been made to our real knowledge of the laws of inorganic matter in this generation. Maxwell, for reasons assigned, assumes that the gaseous molecules repel one another according to a certain law which makes the force insensible, except at very small distances. The theory of elastic molecules involves a similar assumption; for the elasticity of the molecules must be caused by a repulsive action between their atoms, unless we are to accept an elastic molecule as a finality, beyond which our inquiries into the nature of matter cannot extend. The condensable gases and vapours so closely resemble the permanent gases in so many of their properties, that it is impossible not to believe that they are governed by the same laws. If the molecules of hydrogen repel one another at certain distances, we cannot doubt that the same is true with respect to chlorine, and carbonic acid gas, and steam. Indeed it would be difficult to know where to draw the line between hydrogen and the most refractory solid. But if the molecules of carbonic acid gas be brought near enough together, they undoubtedly attract, and at still shorter distances again repel one another. If we assume the existence of isolated atoms, there seems no escape from the doctrine of an alternation of actual attractions and repulsions.

Sir Humphry Davy supposed that the repulsive forces between the particles of matter might be of a nature analogous to that which keeps the planets from falling into the sun, or to what commonly goes by the name of centrifugal force. Except that this view of the case is mentioned with approval by a recent writer, I should not have thought it necessary to make use of any arguments to show that the complicated actions which take place between particles of matter cannot be accounted for by Newton's law of attraction alone. That the particles of a solid body should be not only kept apart, but in permanent general relative positions, by centrifugal force alone, seems to me utterly inconceivable under any known mechanical laws. It may be demonstrated moreover, assuming the theory of atoms, that the cohesive forces of any substance, having any appreciable tenacity, are not only greater, but many millions of times greater than what would be caused by Newton's law. It is so far certain, then, that that law is not absolutely universal, but is replaced or supplemented by something totally different at very short distances.

The atoms or particles of a solid body certainly seem to be in a position of stable equilibrium, or rather to be vibrating about such a position; and there seems no good reason for doubting that such is, not apparently only, but really the case. The following considerations seem to me to show conclusively that it

cannot be the law of nature that two atoms should attract one another for all distances, however small. It seems natural to suppose that the total quantity of heat or energy in any solid body or collection of atoms is finite, and capable of being expressed as some function of the masses, velocities, mutual actions, and relative coordinates of the atoms. Now, if the atoms attract one another for all distances however small, up to actual contact, then the potential energy, or that due to any relative position of the atoms, would depend partly upon the magnitude and internal constitution of the atoms themselves, and would be, humanly speaking, incapable of definite expression. If the atoms be supposed to be mere points or centres of force having inertia but no magnitude, then the potential energy due to the relative position of the atoms of any solid body is infinite. These difficulties disappear if we accept the fact which stares us in the face, that the final action between two atoms when the distance is diminished is repulsive, and that for any two atoms of a solid body there is a relative position of stable equilibrium short of actual contact or coincidence.

Certain considerations in connexion with the dynamical theory of gases require us to suppose that the particles of a gas are not atoms, but molecules or collections of atoms. The facts revealed by the spectroscope indicate also that the particles of vapours, even in their most attenuated condition, have a complicated constitution, and that they probably consist of a considerable number of atoms. Presuming that the mutual action of atoms is the cause of, and of the same kind as that which takes place between molecules, that action may be rather rudely represented by the annexed diagram. The abscissa represents the distance



between two atoms, and the ordinates the force, the positive or-



dinate representing attraction. Supposing one atom fixed at O, A is a position of stable equilibrium for the other, and corresponds to the solid state at the absolute zero of temperature; B is a position of unstable equilibrium, and corresponds to the liquid state. Distances greater than O B correspond to the condition of gas or vapour. It is necessary to suppose that in all cases A B is small compared with O A, and that  $\frac{dy}{dx}$  is large at the point A, but generally small at the point B. I do not know that we have any means of forming an opinion as to the relative magnitude of O C; but such a point as C must exist if we assume the transition from Newton's law to that of gaseous repulsion to be gradual. This necessitates the existence of a maximum negative ordinate D Q. It may be remarked that the distance O D would correspond to the saturation-point at the absolute zero of temperature; for supposing a number of equidistant atoms in a confined space, if the distances be intermediate in magnitude between O B and O D, the equilibrium is unstable, but for distances greater than O D it is stable. For refractory solids E P must be large, and D Q comparatively very small. For gases, A B or E P or both are probably very small, and D Q must be very large compared with its magnitude for non-volatile substances.

If a small velocity be impressed upon the moveable atom in the direction O x, it will oscillate about A. If the initial velocity be increased to a certain value (that is, under the action of a certain amount of heat), the atom will just reach B and there remain. With a greater velocity it will pass beyond B, and the solid connexion between the atoms will be destroyed. It may be seen without any mathematical investigation that the vibratory motion about A produces two effects.

(1) It increases the mean distance between the atoms; that is, it produces expansion.

(2) It diminishes the cohesive force between the atoms; that is, it produces softening.

If the initial velocity be sufficient to carry the atom to B, the cohesive force is entirely destroyed, and the condition is that of perfect liquidity, the mean distance between the atoms being then O B. It may be observed that the greater the initial velocity, and the more nearly in consequence the atom approaches B (the position of unstable equilibrium), the greater is the proportion of energy in the potential or latent state. If the atom just reaches B, the whole energy becomes latent.

With reference to the obvious objections that the liquid state, as thus represented, is one of absolute instability, and that the whole of the heat appears to become latent, I must remark, in

the first place, that probably the liquid condition cannot exist with any permanence except under the combined effects of temperature and pressure; and in the second place, I must anticipate so much as to say that I hope to succeed in showing that it is probable that the atoms of a solid, under the action of heat, aggregate themselves into molecules, and assume the liquid and gaseous conditions at a far lower temperature than what could correspond to the velocity necessary to carry the atom from A to B. That velocity corresponds, not to the melting-point of the substance, but to the far higher temperature—higher perhaps than any at present existing in the solar system, under which a molecule would be resolved into atoms.

If  $x$  be the distance between the atoms, and  $f(x)$  the dynamical measure of the attraction between them, the conditions which have been stated may be approximately expressed by supposing

$$f(x) = (x - \alpha)(\beta - x)^3 \phi(x),$$

where  $OA = \alpha$ ,  $OB = \beta$ , and  $\phi(x)$  is a function which does not change sensibly within the small limits  $x = \alpha$ ,  $x = \beta$ . Let  $\beta - \alpha = h$ ,  $x = \alpha + z$ ,  $h$  and  $z$  being supposed small compared with  $\alpha$ ; then

$$\begin{aligned} f(x) &= z(h - z)^3 \phi(\alpha + z) \\ &= z(h - z)^3 \phi(\alpha) \text{ nearly.} \end{aligned}$$

Put  $f'(\alpha) = h^3 \phi(\alpha) = m^2$ , then

$$f(x) = m^2 z \left(1 - \frac{z}{h}\right)^3.$$

It must be observed that this is little, if any thing, more than a statement in a mathematical form of some of the most obvious properties of ordinary matter. It remains to be seen whether this statement or assumption is consistent with, and can explain other and more recondite properties.

In order to satisfy approximately the condition that  $f'(\beta) = 0$  must be very small, I have made it zero, giving to  $f(x) = 0$  three roots equal to  $\beta$ . Any odd number of roots would apparently do as well as three, but there are good reasons for believing, as I shall show hereafter, that 3 is the correct index.

The equation of motion is

$$\frac{d^2 z}{dt^2} = -f(x),$$

which may be solved approximately when  $z$  is small compared with  $h$ . Neglecting the last term, the equation may be written

$$\frac{d^2 z}{dt^2} + m^2 z = \frac{3m^2}{h} \left( z^2 - \frac{z^3}{h} \right).$$

When  $t=0$  let  $z=a$ ,  $\frac{dz}{dt}=0$ , where  $\frac{a}{h}$  is small. For a first approximation we have

$$z=a \cos mt.$$

By the usual method of successive approximations,  $z$  may be developed in terms of  $\frac{a}{h}$ . The process requires some care, but involves no particular difficulty; so that it will be sufficient to give the result, which, to the degree of approximation required for the present purpose, is

$$z=a[A \cos cmt + Bp + C \cos 2cmt + Dp^2 \cos 3cmt],$$

where

$$p=\frac{a}{h},$$

$$A=1-p+\frac{55p^2}{32},$$

$$B=\frac{3}{2}\left(1-2p+\frac{147p^2}{16}\right),$$

$$C=-\frac{1}{2}(1-2p),$$

$$D=\frac{9}{32},$$

$$c^2=1-\frac{21p^2}{4},$$

$c$  being a factor nearly equal to unity, which, as in the lunar theory, it is necessary to introduce. In conducting the approximations, the condition  $z=a$  when  $t=0$  is preserved throughout. The last term is not made use of in the final application of the result, but is required in the course of the approximations.

The temperature may be assumed to be proportional to the average *vis viva*, or to

$$\frac{\int_0^{\frac{2\pi}{cm}} \left(\frac{dz}{dt}\right)^2 dt}{\frac{2\pi}{cm}} = \frac{a^2 c^2 m^2}{2} (A^2 + 4p^2 C^2),$$

which, neglecting higher powers of  $p$ ,

$$= \frac{m^2 h^2 p^2}{2} \left(1-2p+\frac{3p^2}{16}\right).$$



This is proportional to  $\tau$ , the absolute temperature; so that we may put

$$p^2 \left( 1 - 2p + \frac{3p^2}{16} \right) = \lambda \tau,$$

where  $\lambda$  is some constant. If  $M$  be the mass of an atom,  $H$  the quantity of heat to each atom,  $J$  the mechanical equivalent of heat,

$$JgH = \frac{Mu^2}{2},$$

where  $u$  is the velocity when  $z=0$ . It may be easily shown that

$$u^2 = m^2 h^2 p^2 \left( 1 - 2p + \frac{3p^2}{2} \right),$$

$$H = \frac{Mm^2 h^2 p^2}{2Jg} \left( 1 - 2p + \frac{3p^2}{2} \right);$$

and approximately,

$$p^2 \left( 1 - 2p + \frac{3p^2}{2} \right) = \lambda \tau + \frac{21\lambda^2 \tau^2}{16};$$

$$\therefore H = \frac{Mm^2 h^2 \lambda \tau}{2Jg} \left( 1 + \frac{21\lambda \tau}{16} \right).$$

If  $s$  be the specific heat at the temperature  $\tau$ , or the quantity of heat necessary to raise a unit of mass through one degree,

$$s = \delta \left( \frac{H}{M} \right) = \frac{m^2 h^2 \lambda}{2Jg} \left( 1 + \frac{21\lambda \tau}{8} \right).$$

If  $\sigma$  be the specific heat at the absolute zero,

$$s = \sigma \left( 1 + \frac{21\lambda \tau}{8} \right) = \sigma (1 + \epsilon \tau),$$

where

$$\lambda = \frac{2Jg\sigma}{m^2 h^2}, \quad \epsilon = \frac{21Jg\sigma}{4m^2 h^2}.$$

The expansion is

$$\frac{\int_0^{\frac{2\pi}{mc}} z dt}{\frac{2\pi}{mc}} = Bap,$$

and the rate of expansion

$$\begin{aligned}\frac{Bap}{\alpha} &= \frac{3p^2h}{2\alpha} \left(1 - 2p + \frac{147p^2}{16}\right) \\ &= \frac{3\lambda h\tau}{\alpha} (1 + 9\lambda\tau) \\ &= e\tau(1 + e_1\tau),\end{aligned}$$

which is in accordance with the known laws of expansion,  $e$  and  $e_1$  being constants.

Suppose a solid body consisting of  $n$  equal atoms, and let  $xyz$  represent a very small displacement of an atom. There will be  $3n$  linear differential equations for the determination of such quantities as  $xyz$ ; and it may be shown that

$$x = \sum a \cos(\mu t + l), \quad y = \sum b \cos(\mu t + l), \quad z = \sum c \cos(\mu t + l),$$

where  $\mu$  and  $l$  have  $3n$  different values which are the same for all quantities, such as  $xyz$ . The displacement being small, and no change in the constitution of the body being supposed to take place, terms involving  $e^t$  or  $e^{-t}$  cannot, from the nature of the case, occur. The whole heat for this atom is proportional to

$$\frac{1}{2} \sum \mu^2 (a^2 + b^2 + c^2);$$

and the heat developed as temperature is proportional to one half the non-periodic terms in

$$\left(\frac{dx}{dt}\right)^2 + \left(\frac{dy}{dt}\right)^2 + \left(\frac{dz}{dt}\right)^2 = \frac{1}{4} \sum \mu^2 (a^2 + b^2 + c^2).$$

It follows, therefore, that for every atom, at very small temperatures, one half the heat is developed as temperature and the remainder is latent. If, then, there be two bodies the masses of whose atoms are  $M$  and  $M_1$  respectively, at the same small temperature the whole heat per atom will be the same for both; and if  $\sigma$  and  $\sigma_1$  be their specific heats at the absolute zero of temperature, we have

$$M\sigma = M_1\sigma_1,$$

which accords with what is called the constancy of the atomic heat of simple substances in the solid state. For such substances we should have  $M\sigma = \kappa$ , where  $\kappa$  is constant. If it were possible for two atoms  $M$  and  $M_1$  to become united into a single atom, and  $s$  were the absolute specific heat of the compound, we should have  $(M + M_1)s = \kappa$ . But when two equivalents are chemically combined, it is found that  $(M + M_1)s = 2\kappa$ ; and if there be  $p$  of one and  $q$  of the other,

$$(pM + qM_1)s = (p + q)\kappa.$$

This is what might be inferred from the above considerations; for  $\frac{pM + qM}{p + q}$  is the average mass of an atom of the compound, and  $\frac{pM + qM_1}{p + q}$  is the average heat per atom to produce a rise of  $1^\circ$  from the absolute zero, and therefore equal to  $\kappa$ .

This subject is very fully treated in a valuable memoir by Kopp in the Philosophical Transactions. He points out that the circumstance that  $\kappa$  is nearly the same for most simple solids, does not indicate necessarily that they are really simple, but that they are of the same order of composition. There is some difficulty in the theory, however; for Kopp remarks that the known change of specific heat with change of temperature is not sufficient to account for the observed differences in the values of  $\kappa$ , even for those substances which nearly satisfy the law. This difficulty disappears, I think, when we observe that the quantity estimated and recorded as the atomic heat is

$$M\sigma(1 + \epsilon\tau),$$

and although  $\epsilon$  is very small it is different for different substances, and  $\tau$  being the absolute temperature is considerable. If we had the means of reducing the observations with certainty to the absolute zero, it is probable that the discrepancies would disappear.

The most general case which I have yet attempted to investigate in connexion with the motion of atoms, is that of  $n$  atoms in a straight line. This is far, of course, from being an arrangement which the atoms of a molecule would really assume and maintain: but it is a theoretically possible combination; and having some generality, its consideration may enable us to form by analogy an idea of the nature of molecular arrangements, and lead the way to something better.

I undertook the investigation originally for the purpose of determining the laws of expansion and change of specific heat, but I have been led to conclusions having reference to phenomena of far greater interest and importance.

The law of force which I have assumed affords a reasonable general explanation of some of the phenomena relating to gases and vapours, such as the change of specific heat with change of temperature, and condensation at the dew-point. It points also to an essential distinction between gases and vapours in the nature of the encounters between the molecules. I must, however, defer the consideration of these and many other questions to some future occasion, briefly stating the principle upon which the change of specific heat, of which the absorption of heat in liquefaction and in vaporization are particular cases, seems to



depend. If any system, not subject to loss of energy by friction or any similar cause, be vibrating about a position of stable equilibrium, the average proportion of the energy in a potential state depends greatly upon whether or not there is any position of unstable equilibrium within, or nearly within, the scope of the motion. When the system passes slowly through such a position, a large proportion of the energy becomes potential; and if the motion constitutes heat, a large proportion of the heat becomes latent. The existence of the position of unstable equilibrium, B, appears to be the chief cause of the various changes which are observed in the specific heats of solids, gases, and vapours.

Let there be  $n$  equal atoms in a straight line acting upon one another according to the law already assumed, and slightly disturbed in that line from their positions of stable equilibrium. Let the displacements at time  $t$  be represented by  $x_1, x_2, \dots x_r, \dots$ . The equations of motion to a first approximation are

$$\frac{1}{m^2} \frac{d^2 x_1}{dt^2} + x_1 = x_2,$$

$$\frac{1}{m^2} \frac{d^2 x_2}{dt^2} + 2x_2 = x_1 + x_3,$$

$$\dots \dots \dots$$

or, putting  $q$  for  $\frac{1}{m^2} \left( \frac{d}{dt} \right)^2 + 2$ ,

$$\left. \begin{aligned} (q-1)x_1 &= x_2, \\ qx_2 &= x_1 + x_3, \\ \dots \dots \dots \\ qx_r &= x_{r-1} + x_{r+1}, \\ \dots \dots \dots \\ (q-1)x_n &= x_{n-1}. \end{aligned} \right\} \dots \dots \dots (1)$$

Let these equations be multiplied by  $f_1, f_2, \dots f_r, \dots$  respectively and added together; and let all the terms disappear from the resulting equation, except that involving  $x_n$ . We must have then

$$qf_r = f_{r-1} + f_{r+1}.$$

This gives us

$$f_r = A \cos (r\theta + B),$$

where

$$q = 2 \cos \theta.$$

The condition that  $x_1$  disappears is

$$(2 \cos \theta - 1)f_1 = f_2,$$

whence  $B = -\frac{1}{2}\theta$ ; and supposing  $f_1=1$ , we have

$$f_r = \frac{\cos(r - \frac{1}{2})\theta}{\cos \frac{1}{2}\theta}.$$

The coefficient of  $x_n$  in the resulting equation is

$$(2 \cos \theta - 1)f_n - f_{n-1} = -\frac{2 \sin n\theta \sin \frac{1}{2}\theta}{\cos \frac{1}{2}\theta},$$

and the equation is

$$-\frac{2 \sin n\theta \sin \frac{1}{2}\theta}{\cos \frac{1}{2}\theta} x_n = 0.$$

In a similar manner we obtain

$$+\frac{2 \sin n\theta \sin \frac{1}{2}\theta}{\cos \frac{1}{2}\theta} x_1 = 0;$$

and since  $2 \cos \theta \cdot x_r = x_{r-1} + x_{r+1}$ ,

$$x_r = A \cos(r\theta + B).$$

The first equation gives  $B = -\frac{1}{2}\theta$ , and therefore

$$x_r = A \cos(r - \frac{1}{2})\theta,$$

$$x_1 = A \cos \frac{1}{2}\theta,$$

$$x_r = \frac{\cos(r - \frac{1}{2})\theta}{\cos \frac{1}{2}\theta} x_1.$$

If we put  $\frac{\pi}{2n} = \gamma$ , we have

$$\sin n\theta = \sin \theta (2 \cos \theta - 2 \cos 2\gamma) (2 \cos \theta - 2 \cos 4\gamma) \dots$$

$$(2 \cos \theta - 2 \cos 2(n-1)\gamma),$$

$$\frac{2 \sin n\theta \sin \frac{1}{2}\theta}{\cos \frac{1}{2}\theta}$$

$$= 4 \sin^2 \frac{1}{2}\theta (-4 \sin^2 \frac{1}{2}\theta + 4 \sin^2 \gamma) (-4 \sin^2 \frac{1}{2}\theta + 4 \sin^2 2\gamma) \dots$$

$$\text{And since } 2 \cos \theta = \frac{1}{m^2} \left( \frac{d}{dt} \right)^2 + 2, \quad -4 \sin^2 \frac{1}{2}\theta = \frac{1}{m^2} \left( \frac{d}{dt} \right)^2, \text{ the}$$

equation for determining  $x_1$  becomes

$$\frac{d^2}{dt^2} \left( \frac{d^2}{dt^2} + \mu_1^2 \right) \dots \left( \frac{d^2}{dt^2} + \mu_s^2 \right) \dots \left( \frac{d^2}{dt^2} + \mu_{n-1}^2 \right) x_1 = 0, \quad (2)$$

where  $\mu_s = 2m \sin s\gamma$ ,  $s$  being any number from 1 to  $n-1$ .

If  $\frac{dx_r}{dt} = 0$  when  $t=0$ , for all values of  $r$  we have

$$x_1 = a + a_1 \cos \mu_1 t + \dots + a_s \cos \mu_s t + \dots + a_{n-1} \cos \mu_{n-1} t,$$

where  $a, a_1, \dots a_s$  are arbitrary constants. It may be easily shown that  $a$  is the same for all values of  $r$ , since

$$x_r = \frac{\cos(r - \frac{1}{2})\theta}{\cos \frac{1}{2}\theta} x_1,$$

and that, when operating upon  $\cos \mu_s t$ ,  $\theta = 2s\gamma$ ;

$$x_r = a + \sum_{s=1}^{s=n-1} a_s \frac{\cos(2r-1)s\gamma}{\cos s\gamma} \cos \mu_s t. \quad \therefore \quad (3)$$

If  $x_r = 0$  when  $t = 0$ , for all values of  $r$  we find in the same way

$$\dot{x}_r = bt + \sum_{s=1}^{s=n-1} b_s \frac{\cos(2r-1)s\gamma}{\cos s\gamma} \sin \mu_s t, \quad \dots \quad (4)$$

$r$  being the number of the atom,  $s$  the number of the term in  $x_r$ , and  $b, b_1, \dots$  arbitrary constants.

Suppose the initial conditions to be

$$x_r = \phi(r), \quad \frac{dx_r}{dt} = 0,$$

where  $\phi$  is of any given form. For the determination of the arbitrary constants, we have  $n$  equations of the form

$$\phi(r) = a + \sum_{s=1}^{s=n-1} a_s \frac{\cos(2r-1)s\gamma}{\cos s\gamma}. \quad \dots \quad (5)$$

It may be shown that, if  $p$  and  $q$  be any two integers,

$$\sum_{r=1}^{r=n} \cos(2r-1)p\gamma \cos(2r-1)q\gamma = 0,$$

except when  $p = q$ , when the sum is  $\frac{n}{2}$ . If, then, the equations of the form (5) be multiplied respectively by  $\cos s\gamma, \cos 3s\gamma, \dots \cos(2r-1)s\gamma \dots$  and added together, all the terms on the right-hand side disappear except those involving  $a_s$ , and we have

$$\sum_{r=1}^{r=n} \phi(r) \cos(2r-1)s\gamma = \frac{na_s}{2 \cos s\gamma};$$

and adding together equations (5) as they stand,

$$\sum \phi(r) = na,$$

$$\therefore a = \frac{1}{n} \sum \phi(r)$$

$$+ \frac{2}{n} \sum_{s=1}^{s=n-1} \sum_{r=1}^{r=n} (\phi(r) \cos(2r-1)s\gamma) \cos(2r-1)s\gamma \cos \mu_s t. \quad (6)$$

If the initial conditions be  $x_r = 0, \frac{dx_r}{dt} = \phi_1(r)$ , then, from



equation (4),

$$\phi_1(r) = b + \sum b_s \mu_s \frac{\cos(2r-1)s\gamma}{\cos s\gamma}.$$

These equations give, as before,

$$\sum_{r=1}^{r=n} \phi_1(r) \cos(2r-1)s\gamma = \frac{n\mu_s b_s}{2 \cos s\gamma},$$

$$\sum \phi_1(r) = nb;$$

$$\therefore x_r = \frac{1}{n} \sum \phi_1(r) t$$

$$+ \frac{2}{n} \sum_{s=1}^{s=n-1} \Sigma (\phi_1(r) \cos(2r-1)s\gamma) \frac{\cos(2r-1)s\gamma}{\mu_s} \sin \mu_s t, \quad (7)$$

which completes the solution to a first approximation.

It may be observed that the formulæ given by Poisson for the longitudinal vibrations of an elastic rod may be easily deduced from the above results. It is remarkable also that, by putting  $t=0$  and  $x_r = \phi(r)$  in equation (6), a general analytical theorem may be deduced, of which Lagrange's theorem, that when  $\phi(0)=0$  and  $\phi(a)=0$ ,

$$\phi(x) = \frac{2}{a} \sum_0^\infty \left( \int_0^a \phi(x) \sin \frac{n\pi x}{a} \right) \sin \frac{n\pi x}{a}$$

is a particular case.

In order to determine how the system would vibrate if disturbed and then left to itself, suppose the first atom to receive a blow impressing upon it a velocity  $ma$ , which, if the next atom were fixed, would cause it to vibrate through a space  $a$  nearly,  $a$  being small compared with  $h$ . We have then  $\phi(r)=0$ ,  $\phi_1(r)=ma$  when  $r=1$ , and zero for all other values.

$$\Sigma \phi_1(r) \cos(2r-1)s\gamma = ma \sin s\gamma,$$

$$\Sigma \phi_1(r) = ma,$$

$$x_r = \frac{mat}{n} + \frac{a}{n} \sum_{s=1}^{s=n-1} \sin(2r-1)s\gamma \sin \mu_s t.$$

The nature of the vibrations will be the same, and the term  $\frac{mat}{n}$ , denoting the general motion of translation of the system, be avoided, by supposing the initial conditions to be

$$\phi(r) = \frac{a}{n} \Sigma \sin(2r-1)s\gamma, \quad \phi_1(r) = 0;$$

then

$$x_r = \frac{a}{n} \sum_{s=1}^{s=n-1} \sin(2r-1)s\gamma \cos \mu_s t.$$

Since  $2n\pi=\gamma$ , it is evident that

$$x_{n-r+1} = -x_r;$$

so that the motion is symmetrical about the middle point. If  $n$  be an odd number the central atom will remain at rest. These conclusions hold for the higher approximations as well as for the first.

The whole energy is  $\frac{m^2 a^2}{2}$ ; and the vibratory energy, which alone is represented in the above value of  $x_r$ , is  $\frac{n-1}{2n} m^2 a^2$ . The heat developed as temperature for the  $r$ th atom is

$$\frac{m^2 a^2}{n^2} \sum \sin^2 s\gamma \sin^2 (2r-1)s\gamma = \frac{m^2 a^2 (n-1)}{4n^2},$$

and is the same for all, the mass of an atom being here supposed to be unity.

Subject to no external disturbance whatever, any number of atoms might vibrate together in the manner indicated; but this is a condition which can never exist; for the atoms of a molecule of vapour, even during the interval between their encounters, are subject to acceleration or retardation, as the case may be, from the action of the æther in which they must be supposed to be immersed. A notion may be formed of the effect of an external disturbance upon such a system as that under consideration, by supposing an additional atom at the beginning of the series constrained to move according to a particular law. Let  $x_0$  be the displacement of this atom, and suppose

$$x_0 = a \cos \mu t,$$

$a$  being small compared with  $h$ . The equations of motion are

$$\begin{aligned} qx_1 &= x_0 + x_2, \\ &\dots \dots \dots \\ qx_r &= x_{r+1} + x_{r-1}, \\ &\dots \dots \dots \\ (q-1)x_n &= x_{n-1}, \end{aligned}$$

where, as before,

$$q = 2 + \frac{1}{m^2} \left( \frac{d}{dt} \right)^2 = 2 \cos \theta.$$

If these equations be multiplied by  $f_1, f_2, \dots$  respectively and added together, and all the terms disappear from the resulting equation except those involving  $x_0$  and  $x_n$ , we have

$$2 \cos \theta f_r = f_{r-1} + f_{r+1},$$

which gives  $f = A \sin (r\theta + B)$ .

The condition  $2f_1 \cos \theta = f_2$  gives  $B=0$ ; so supposing  $f_1=1$ , we have

$$f_r = \frac{\sin r\theta}{\sin \theta},$$

and the coefficient of  $x_n$  is

$$(2 \cos \theta - 1)f_n - f_{n-1} = \frac{\cos(n + \frac{1}{2})\theta}{\cos \frac{1}{2}\theta}.$$

The equation is therefore

$$\frac{\cos(n + \frac{1}{2})\theta}{\cos \frac{1}{2}\theta} x_n = a \cos \mu t.$$

We have also

$$x_r = A \cos(r\theta + B);$$

and the condition  $(2 \cos \theta - 1)x_n = x_{n-1}$  gives  $B = \frac{2n+1}{2}\theta$ ; so that

$$x_r = A \cos(n - r + \frac{1}{2})\theta,$$

$$x_n = A \cos \frac{1}{2}\theta,$$

$$\begin{aligned} x_r &= \frac{\cos(n - r + \frac{1}{2})\theta}{\cos \frac{1}{2}\theta} x_n \\ &= a \frac{\cos(n - r + \frac{1}{2})\theta}{\cos(n + \frac{1}{2})\theta} \cos \mu t. \end{aligned}$$

If we put  $\mu = 2m \sin \psi$ , then, when operating upon  $\cos \mu t$ ,  $\theta = 2\psi$ , and

$$x_r = a \frac{\cos(2n - 2r + 1)\psi}{\cos(2n + 1)\psi} \cos \mu t.$$

The tendency to rupture is a function, not of the displacements, but of the relative displacements of the atoms, represented by

$$x_r - x_{r-1} = \frac{2a \sin(2n - 2r + 2)\psi \sin \psi}{\cos(2n + 1)\psi} \cos \mu t.$$

If  $n$  be considerable, but  $\psi$  so small that  $(2n+1)\psi$  is not nearly equal to  $\frac{\pi}{2}$ ,  $\sin \psi$  will be small, and the relative disturbance small compared with  $a$  for all values of  $r$ . This shows that a slow external disturbance, corresponding to a small value of  $\mu$ , will cause a general oscillatory motion of the whole system, but very little internal relative vibration of the atoms.

If  $(2n+1)\psi$  be equal to  $\frac{\pi}{2}$ , or to any odd multiple of it,  $x_r - x_{r-1}$  becomes infinite, indicating the well-known change in the form of the solution from  $A \cos \mu t$  to  $At \sin \mu t$ . I will defer the consideration of this particular case, and suppose  $(2n+1)\psi$



to be nearly equal to some odd multiple of  $\frac{\pi}{2}$ , so that  $\cos(2n+1)\psi$  is small, but  $a$  so small that the character of the vibrations is maintained. Since

$$\sin(2n-2r+2)\psi = \cos(2r-1)\psi \text{ nearly,}$$

the relative displacement is a maximum and large compared with  $a$  for such values of  $r$  as make  $(2r-1)\psi$  a multiple of  $\pi$  or most nearly so; and this, of course, occurs at regular intervals. If  $p$  be the whole number most nearly satisfying the condition  $2p\psi = \pi$ , the atoms are arranged in groups of  $p$  each, where  $p$  is a number depending upon the wave-length of the disturbance and the nature of the system, and not at all upon  $a$  or the intensity of the disturbance. Suppose now  $a$  or the temperature to increase gradually; the groups remain the same, but become more and more isolated. Each group acquires an oscillatory motion as a whole in addition to the vibratory motion of its atoms amongst themselves; and the time of this oscillation corresponds nearly to the fundamental or lowest note of a group of  $p$  atoms vibrating without restraint; for in that case

$$\mu_1 = 2m \sin \frac{2\pi}{p} = 2m \sin \psi = \mu \text{ nearly.}$$

The disturbance  $a \cos \mu t$  representing the prevailing heat,  $\mu$  and therefore  $\psi$  also, for atoms of a given kind, has a certain very limited range of value. We may suppose, then, that there is some one particular value of  $\psi$  making  $\frac{\pi}{2\psi} = p$  an integer; and that particular wave-length corresponds exactly to the fundamental note of the group, or molecule, of  $p$  atoms.

If we suppose now that  $a$  increases until the maximum value of the relative displacement exceeds a certain quantity, the severance becomes complete. This does not necessarily take place at all points at once; for the weakening of the connexion between the groups would impede the propagation of the disturbance. The first group would first melt off, or, if the temperature were higher, would fly off as a molecule of vapour; and the next group would then be directly exposed to the disturbance, and be melted or evaporated in its turn. It should be observed that the energy employed in severing the connexion between the groups does not increase the temperature, but becomes latent. I must remark here that the liquid condition would be better accounted for, and some other phenomena perhaps explained, by supposing the three roots equal to  $\beta$  of the equation  $f(x)=0$  to be replaced by three roots nearly equal to each other—that is, roots whose differences are generally small compared with  $h$ . If

it be objected that I am assuming an unnaturally complicated and fantastic law, I can only repeat that it is not assumed arbitrarily, but is little more than a reflex of plain facts. If all the various phenomena relating to inorganic matter are to be accounted for by the motions of atoms acting upon one another according to some one law (and this assumption must be the foundation of every such attempt as the present), it is hardly reasonable to suppose that the law of action which is to be the cause of such a vast variety of complex relations will be of a very simple kind.

Having considered the motion of the system whilst being heated as it were, let us now consider what would occur if the disturbance were to cease and the system be left to vibrate of itself. When  $\mu t$  is any multiple of  $2\pi$ ,

$$x_r = a \frac{\cos(2n-2r+1)\psi}{\cos(2n+1)\psi}, \frac{dx_r}{dt} = 0.$$

At any such instant let the disturbing atom be removed, and we have for the subsequent motion the initial conditions

$$\phi(r) = a \frac{\cos(2n-2r+1)\psi}{\cos(2n+1)\psi}, \phi_1(r) = 0.$$

We suppose that the system is an aggregation of molecules, formed as above described, under the action of the prevailing heat; so that  $n$  is a multiple  $pv$  of  $p$ ; and if  $2p\psi = \pi$  exactly, we have

$$2n\psi = v\pi, \phi(r) = a \frac{\cos(2r-1)\frac{\pi}{2p}}{\cos\frac{\pi}{2p}} = a \frac{\cos(2r-1)v\gamma}{\cos v\gamma}.$$

Referring to equation (6),  $\Sigma\phi(r) = 0$ ,

$$\Sigma_{r=1}^{r=n} \phi(r) \cos(2r-1)s\gamma = \frac{a}{\cos v\gamma} \Sigma \cos(2r-1)v\gamma \cos(2r-1)s\gamma,$$

which vanishes for all values of  $s$  except  $s=v$ , and in that case is  $\frac{an}{2\cos v\gamma}$ ,

$$x_r = a \cos(2r-1)v\gamma \cos \mu_v t,$$

$$\mu_v = 2m \sin v\gamma = 2m \sin \psi = \mu,$$

$$x_r = a \frac{\cos(2r-1)\psi}{\cos \psi} \cos \mu t;$$

or the motion continues in this case to be the same exactly as that impressed upon the system by the disturbance. The system in cooling would radiate the same kind of heat as that which it

received; that is, it would give back the fundamental note of its molecules quite pure.

If it shall hereafter appear that we are justified in inferring that atoms under any natural arrangement, under the action of heat of a certain wave-length, would behave in a manner analogous to that which they appear to adopt when constrained to move in a straight line, then I think it will be found that we have fallen upon a principle of great importance in the economy of nature. It may be briefly stated thus. The arrangement of atoms in a molecule is caused by the prevalent heat, and depends upon its wave-length; and every molecule generated under the action of heat of a certain wave-length, radiates heat of the same or nearly the same wave-length. I do not consider, of course, that the existence of this principle is proved, or that these investigations afford us any thing more perhaps than a hint of the truth. In the further remarks which I shall have to make I shall, however, assume the truth of the principle which I have stated; but I hope that it will be understood, if I appear to adopt too confident a tone, that I do so merely to avoid the awkward recurrence of a hypothetical mode of expression.

Before indulging in any speculations, I must dwell a little longer upon the dry formulæ. We will endeavour to form some general idea of the value of  $\psi$  from the equations  $\mu = 2m \sin \psi$ ,  $2p\psi = \pi$ .  $\mu$  we know is very large; and  $m$  for ordinary solids must also be very large, for the molecular forces are enormous in relation to the masses upon which they act. If  $\psi = \frac{\pi}{4}$ , then

$p = 2$ ; if  $\psi$  is greater than  $\frac{\pi}{3}$ ,  $p = 1$ , or the supposed molecular arrangement would not occur. If there be any simple substance for which  $\psi$  is greater than  $\frac{\pi}{3}$ , it must exist in the state of in-

dependent atoms and be incapable of assuming the liquid state, except perhaps under great pressure; if heated, it would pass at once from the solid to the vaporous state. It would show no bright lines in the spectrum at any temperature. In a state of vapour no heat could be consumed in internal vibrations; so that Maxwell's factor  $\beta$  would in this case be unity. I am not aware that there is any substance possessing these properties; but at all events we may presume, from the complicated constitutions which molecules appear to possess, that  $p$  is generally considerable and  $\psi$  therefore small. The above equations give us

$$\frac{\delta p}{p} = -\frac{\delta \psi}{\psi} = -\frac{\delta \mu}{\mu \psi \cot \psi}.$$



$\delta\mu$  represents the range of  $\mu$  in the prevalent heat; and for heat of considerable intensity  $\frac{\delta\mu}{\mu}$  is small; and  $\psi$  being small,  $\frac{\delta\psi}{\psi}$  does not differ much from  $\frac{\delta\mu}{\mu}$ . If for any substance  $\psi$  were small enough,  $\delta p$  might exceed unity, even within the range of heat of considerable intensity, in which case there would be different values of  $p$  for different wave-lengths. The molecular arrangements of such a substance would display great instability. A molecule formed under one wave-length would be decomposed and otherwise arranged by heat of a different kind. As we have every reason to believe that this kind of instability does not exist in the case of simple substances, we may infer that  $\psi$  is not very small, the general conclusion being that for ordinary substances  $\psi$  is small but not very small.

The particular value of  $\psi$  which for any substance is equal to  $\frac{\pi}{2p}$  is not the one which produces the greatest effect in arranging the atoms into groups. The greatest effect is caused by the value of  $\psi$  which makes  $(2n+1)\psi$  equal to an odd multiple of  $\frac{\pi}{2}$ ; and when  $n$  is large, such a value must exist. In this case the expression for the relative displacement becomes infinite. This does not indicate that there would necessarily be a rupture of the system, but merely that the displacement cannot be expressed exactly in the manner supposed. The best way of stating the case is, that a vibration of that exact wave-length cannot be propagated through the system at all. By the effect of the factor  $c$ , by which in the second approximation it becomes necessary to multiply  $\mu$ , the time of vibration and the wave-length are increased; so that if  $\psi$  be continuous, this particular vibration will be assimilated to, and fall in with that corresponding to some smaller value of  $\psi$ . As  $a$  increases, this effect increases and extends to values of  $\psi$  nearly equal (within a certain range) to the particular value in question.

The temperature of the  $r$ th atom in the case under consideration is

$$\frac{\mu^2 a^2 \lambda}{4} \frac{\cos^2 (2n-2r+1)\psi}{\cos^2 (2n+1)\psi},$$

$\lambda$  being some constant; and the average temperature is

$$\frac{\mu^2 a^2 \lambda}{4n} \sum \frac{\cos^2 (2n-2r+1)\psi}{\cos^2 (2n+1)\psi} = \frac{\mu^2 a^2 \lambda}{8 \cos^2 (2n+1)\psi} \text{ nearly}$$

when  $n$  is large. The maximum temperature is

$$\frac{\mu^2 a^2 \lambda}{4 \cos^2 (2n+1) \psi}.$$

There is thus a concentration of heat at the joints; so that at those points a greater softening takes place than would occur if the heat were uniformly distributed. This effect is increased as the temperature increases, on account of the increasing proportion of the heat employed in softening the joints, which becomes latent. The temperature, therefore, at which the melting occurs is much lower than what, if uniformly distributed, would dis sever the atoms.

The effect of radiant heat upon a system of  $n$  atoms, such as that under consideration, may perhaps be represented by supposing the additional atom at the beginning of the series to be of feeble power, proportional to  $\lambda m^2$ , when  $\lambda$  is very small. Let  $x_0$  be the displacement of this atom, and

$$x_0 = a \cos \mu t;$$

the equation of motion for the first of the  $n$  atoms is

$$\frac{d^2 x_1}{dt^2} = m^2 (x_2 - x_1) + \lambda m^2 (x_0 - x_1),$$

or

$$\frac{1}{m^2} \frac{d^2 x_1}{dt^2} + (1 + \lambda) x_1 = \lambda x_0 + x_2;$$

neglecting  $\lambda$  in comparison with 1, and putting

$$\frac{1}{m^2} \left( \frac{d}{dt} \right)^2 + 2 = q = 2 \cos \theta,$$

we have

$$(q-1)x_1 = x_2 + \lambda x_0,$$

$$qx_2 = x_1 + x_3,$$

$$\dots \dots \dots$$

$$(q-1)x_n = x_{n-1}.$$

These equations give

$$\begin{aligned} -\frac{2 \sin n\theta \sin \frac{1}{2}\theta}{\cos \frac{1}{2}\theta} x_n &= \lambda x_0 = \lambda a \cos \mu t x_r = \frac{\cos(n-r+\frac{1}{2})\theta}{\cos \frac{1}{2}\theta} x \\ &= -\frac{\lambda a \cos(n-r+\frac{1}{2})\theta \cos \mu t}{2 \sin n\theta \sin \frac{1}{2}\theta}; \dots \dots \dots (8) \end{aligned}$$

and the relative displacement is

$$x_{r+1} - x_r = -\frac{\lambda a \sin(n-r)\theta \cos \mu t}{\sin n\theta}.$$

Let  $\mu = 2m \sin \psi$ ; then  $\theta$ , operating upon  $\cos \mu t$ , is equal to  $2\psi$ , and

$$x_{r+1} - x_r = - \frac{\lambda a \sin 2(n-r)\psi \cos \mu t}{\sin 2n\psi}.$$

Now  $\lambda$ , representing the ratio of the action of an atom of æther to the mutual action of the atoms of a solid body, is very small—almost infinitesimal. The above coefficient of  $\cos \mu t$  is therefore wholly insignificant except when  $\sin 2n\psi$  is very small, or when  $2n\psi =$  a multiple of  $\pi$ . In this case, as before explained, the quantity by which  $\lambda$  is multiplied is large, but not infinite; for as the amplitude of the vibration increases, the time of vibration is slightly increased, as indicated by the factor  $c$  introduced in the second approximation; and  $\psi$  is thus slightly diminished. The only values of  $\psi$  which produce any sensible effect are  $\frac{\pi}{2n}, \frac{2\pi}{2n}, \frac{3\pi}{2n}, \&c. \dots$  Let  $\psi = \frac{v\pi}{2n} = v\gamma$ , where  $v$  is an integer;

putting  $b = \frac{\lambda a}{2 \sin n\theta}$ , we have, supposing the  $n$  atoms to have been initially at rest in their positions of stable equilibrium,

$$x_r = a_1 + \sum_r a_s \cos \mu_s t - \frac{b \cos (2n - 2r + 1)\gamma}{\sin v\gamma} \cos \mu t,$$

where  $a_1, a_s$  are arbitrary constants, and  $\mu_s = 2m \sin s\gamma$ . Putting  $t = 0$ ,

$$0 = a_1 + \sum_r a_s - \frac{b \cos (2n - 2r + 1)v\gamma}{\sin v\gamma}.$$

Equation (8) gives

$${}_r a_s = {}_n a_s \frac{\cos (2n - 2r + 1)s\gamma}{\cos s\gamma};$$

whence it may be shown that  ${}_n a_s$ , and consequently  ${}_r a_s$ , vanishes for all values of  $s$  except  $s = v$ , and

$$\begin{aligned} \frac{{}_n a_v}{\cos v\gamma} &= \frac{b}{\sin v\gamma} \\ x_r &= \frac{b \cos (2n - 2r + 1)v\gamma}{\sin v\gamma} (1 - \cos \mu t) \\ &= \frac{b \cos (2n - 2r + 1)\psi}{\sin \psi} (1 - \cos \mu t). \end{aligned}$$

If the disturbance be supposed to cease, it may be shown, as before, that the subsequent motion is represented by

$$x_r = \frac{b \cos (2n - 2r + 1)\psi}{\sin \psi} \cos \mu t;$$

so that the system radiates the same kind of heat which it absorbs. If  $\mu$  be greater than  $2m$ ,  $x_r$  is small, even in comparison



with  $\lambda a$ , when  $n$  is large; so that the system is incapable of absorbing or transmitting any heat for which  $\mu$  is greater than  $2m$ .

Equation (8) gives

$$x_1 = \frac{b \cos (2n-1) \psi}{\sin \psi} (1 - \cos \mu t).$$

So long as this coefficient is greater than  $a$ , the first of the  $n$  atoms continues to be accelerated by the æther. When steady motion is established and the acceleration ceases, we have

$$\begin{aligned} \frac{b \cos (2n-1) \psi}{\sin \psi} &= a, \\ x_r &= \frac{a \cos (2n-2r+1) \psi}{\cos (2n-1) \psi} (1 - \cos \mu t) \\ &= \frac{a \cos (2r-1) \psi}{\cos \psi} (1 - \cos \mu t). \end{aligned}$$

The principle which I have stated, if established, would afford some hope of our being able to understand the facts of "spectrum analysis." The fixed character of the bright lines makes it impossible to conceive that the energy due to the translation or rotation of the vapour molecules can have any thing to do with their production. They must be caused by the internal vibrations of something of which the molecule is composed. A complete knowledge of the arrangement and mode of vibration of the atoms of a molecule would involve a complete knowledge of the corresponding bright lines. We may imagine that the molecule of the very simple structure which we have been considering would give one bright line, corresponding to its fundamental note; and fainter lines would correspond to some of the terms of the second and higher approximations.

It can hardly, I think, have escaped notice, that if the mean translation velocity of the molecules of an incandescent vapour become so great as to bear a sensible ratio to that of wave-propagation, the wave-length of the light corresponding to any bright line will be affected in a manner and degree depending upon the direction of motion relative to that of the light observed. As the temperature is gradually increased, this will have the effect of thickening the bright lines, and finally of converting them into a continuous spectrum. If, as the temperature is increased, a rupture or change of constitution should take place in the molecules, we may expect a sudden change in the appearance of the spectrum. In reference to this subject, I may remark, although I express an opinion with much hesitation, knowing how much there is which has been written upon this subject which I have not had an opportunity of studying, that

I believe that the constitution of the luminiferous æther is such as to render it incapable of propagating waves of less than a certain length.

I see some hope also of an explanation of what has always appeared to me one of the greatest difficulties in connexion with molecular physics—that the wave-length should be so nearly the same for all kinds of heat. It is not difficult to conceive that the molecules in the sun and elsewhere, whose vibrations are the chief sources of heat, should have been so constituted as to vibrate nearly in the same time. The difficulty is to understand why the molecules of bodies of all kinds and constitutions, being heated and then left to vibrate in their own way, should all vibrate so nearly in unison. But if we hold that the arrangement of atoms into molecules is caused by the prevalent heat and depends upon its wave-length, the difficulty disappears. Let us suppose for a moment that the sun should radiate heat of one uniform wave-length only, and that the values of  $m$  for all substances and combinations were such that  $\frac{\pi}{2\psi}$  were in every case

a whole number exactly. All the atoms under the influence of the sun's heat would be arranged into molecules, all having the same fundamental note, and collections of such molecules, after being heated, would give back that note alone. No substance having its atoms otherwise arranged could continue to exist; for every ray of heat which it encountered would assist in decomposing and rearranging its atoms according to the prevailing code. There would be one uniform stability of molecular constitution and one uniform colour.  $\mu$  appears to be as nearly constant as the necessity that  $p$  should be a whole number allows. If  $\mu$  were not nearly constant for heat of considerable intensity, there would be no stability in the constitution of matter; for an arrangement made under one wave-length would be liable to be decomposed under another. Suppose that a mass of any substance, such as iron, were brought from some other system, if there is any such, where a much longer wave-length prevails; we should probably not recognize it as iron at all. If melted, its atoms would be immediately arranged according to the fashion of our system. It might perhaps be preserved in its original state if carefully kept in a cool place. If exposed to the heat of the day, it would probably be gradually transformed, suffering disintegration in the process; it would decay, in fact, much as a piece of wood does, and with more or less rapidity, according to the degree in which its constitution differed from our standard. Is it possible that organic compounds, which can be produced and exist under exceptional circumstances only, which are so liable to decay, so sensitive to the action of

heat, and differ sometimes so entirely from inorganic substances formed of the same chemical elements, may involve abnormal molecular arrangements not in accordance with the prevailing wave-length, and thus be liable to decay when removed from the local influences under which they were produced?

The present uniformity of wave-length is a condition of dynamical equilibrium, which may have existed from the beginning, but which may, I think, have been brought about by the operation of natural causes. Supposing a number of atoms, enough to make a solar system, to have been created anywhere in space, but at such distances apart as to cause by their confluence, a sufficient amount of heat to animate the whole. In the beginning there would be a true chaos. There would be every variety of wave-length, and consequently every variety of molecular arrangement with no stability anywhere, but a continuous process of composition and decomposition. But out of this chaos order would be gradually evolved. The principle of natural selection would begin to operate even at this early period. Every radiating molecule would endeavour to impress its own constitution upon others within its influence, to propagate its kind. In the warfare among the molecules every enemy conquered would become the ally of the conqueror. The molecules distinguished by numbers and strength of constitution would gradually gain the ascendancy by the destruction of weaker kinds; and any additional stability of structure which might accidentally arise amongst themselves would be propagated and become general. An ascendancy having once been gained, the process of reduction to a common standard would go on with an ever increasing rapidity until the condition of greatest stability was attained. If such a relation existed amongst the constants upon which the mutual action of the atoms depends, as to render it possible that one uniform wave-length should be attained, that would be the final result. In that case there would be one uniform stability of molecular arrangement—a hard uncompromising state of things, without the possibility perhaps of that continuous round of composition and decomposition upon which the life of our part of the universe depends.

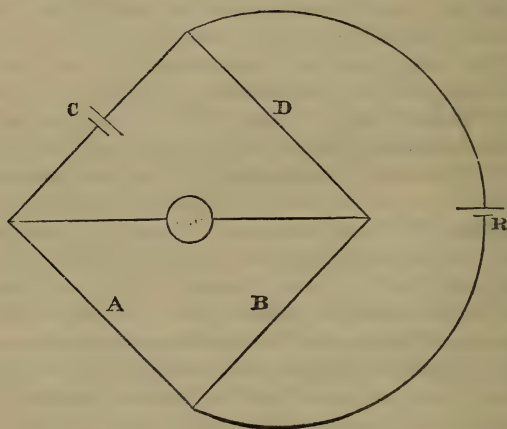
It may be, then, that chaos means diversity of wave-length, and that cosmos means variety in unity, and that absolute uniformity of wave-length would be universal death. It is a curious subject for reflection, that the possibility of cosmos evolving out of chaos (that is, the possibility that the material universe should become fitted to be the abode of organic life) may have depended upon whether or not a few constants were so arranged in the beginning as to satisfy a simple mathematical condition.



**XX. On Testing the Metal-resistance of Telegraph-wires or Cables influenced by Earth-currents.** By G. K. WINTER, *Telegraph Engineer, Madras Railway\**.

[With a Plate.]

**T**HE fact, I believe, is sufficiently well known to all who have had any thing to do with the testing of telegraph-wires or cables, that it is almost impossible to have two earth-plates inserted any distance apart without a difference of tension, greater or less according to circumstances, existing between them; this is due in some cases to earth-currents properly so called, in others to polarization of the earth-plates from the passage of currents, in others to a difference between the earth-plates themselves or the soil in which they are imbedded, but generally to these causes combined. The important influence of the currents in the wires due to this difference of tension upon the apparent resistance of the wire when tested by the Wheatstone's bridge has not, I fear, been hitherto fully appreciated. In Sabine's 'The Electric Telegraph,' pp. 292 and 293, the author deduces from Kirchhoff's laws the following equations (see the annexed diagram) :—



$$\pm \frac{E'}{E} = \frac{BC - AD}{A(D + R) + B(A + R)}, \quad \dots \dots (1)$$

$$C = \frac{AD}{B} \pm \frac{E'}{E} \frac{A(D + R) + B(A + R)}{B}, \quad \dots \dots (2)$$

in which E is the electromotive force of the testing battery, and E' the foreign electromotive force in the branch C.

\* Communicated by the Author.

From equation (1) we can tell the ratio between these forces when the resistance of C is known; and from equation (2) the resistance of C is found when the ratio  $\frac{E'}{E}$  is known; but as these equations are derived one from the other, we cannot, with these equations alone, eliminate either of the unknowns. The author has omitted to point out that if we take two tests, one with copper to line and the other with zinc to line, and the results obtained be called D and d, then

$$\pm \frac{E'}{E} = \frac{BC - AD}{A(D + R) + B(A + R)}, \quad \dots \quad (3)$$

$$\mp \frac{E'}{E} = \frac{BC - Ad}{A(d + R) + B(A + R)}, \quad \dots \quad (4)$$

the signs before the ratio  $\frac{E'}{E}$  being opposite in the two equations, but the decision as to which is *plus* and which *minus* depending upon the direction of the foreign electromotive force, which we shall henceforth call the earth-current.

It follows from this that if we add the two equations together we at once eliminate  $\frac{E'}{E}$ , and get

$$\frac{BC - AD}{A(D + R) + B(A + R)} + \frac{BC - Ad}{A(d + R) + B(A + R)} = 0, \quad (5)$$

from which C is easily calculated, especially when (as is usually the case in testing line-resistance)  $A = B$ . The equation then becomes

$$\frac{C - D}{D + 2R + A} + \frac{C - d}{d + 2R + A} = 0. \quad \dots \quad (6)$$

Again, if we omit R (the resistance of the testing battery), we get

$$\frac{C - D}{D + A} + \frac{C - d}{d + A} = 0. \quad \dots \quad (7)$$

As, however, the battery-resistance is seldom so small as to be neglected, it is generally better to use equation (6).

In Clark and Sabine's 'Electrical Tables and Formulæ,' and in a pamphlet issued to the Indian Government Telegraph Department, is a formula by Schwendler\* which, while really iden-

\* The equation as given by Schwendler is as follows:—

$$x = \frac{bf(a+b)(W' + W'') + b^2(aW' + 2W'W'' + aW'')}{ab(W' + W'') + 2af(a+b) + 2a^2b},$$

tical with equation (5), is much more complicated and tedious to solve, owing to the attempt to isolate the unknown quantity in the literal equation, which is, I think, a mistake in this case.

The above correction to the Wheatstone-bridge method of testing line-resistance, however, is only true so long as the ratio  $\frac{E'}{E}$  remains constant during the two tests. We have not much fear of the electromotive force of the testing battery changing; but earth-currents are, as a rule, continually varying in strength; for this reason I have always considered the bridge method of testing line-resistance unreliable. There are at least two methods which are much to be preferred. One is Mance's method, given in the Number of this Magazine for April last (p. 314), and the other a modification of Varley's method of testing the internal resistance of a cell by the differential galvanometer. In both these methods the cable or line, together with its two earth-plates, is considered as an electromotor, and its internal resistance is measured.

I need scarcely say that in every method of measuring the internal resistance of an electromotor, unless its electromotive force is known, two observations are necessary; and if this force is liable to vary, the more quickly these observations follow each other the greater the chance of accuracy. In the two tests just referred to, the first observation is simply the noting of a deflection, and the second is simply seeing that the deflection does not alter when we press a key in one case and raise a key in the other. In land-lines there is usually no interval at all between the two observations; but in cables the alteration in the resistance of the circuit causes a sudden difference in the dynamic charge of the cable, and hence a sudden flow through the galvanometer. We have therefore merely to wait till the needle comes to rest before the second observation can be taken. Mance's method was so fully described in the April Number of this Journal that I need not further refer to it; so I will proceed at once to the second method, which is more applicable to measuring the resistance of land-lines.

The instrument I use for the purpose is a double-shunt differential galvanometer having the resistance and magnetic effect of its coils equal. The connexions will be readily understood from fig. 2 (Plate II.). C is a thick wire offering comparatively no resistance. The key is first pressed, so that the earth-current flows through only one coil of the galvanometer, and the

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in which  $a$  and  $b$  are the branch resistances,  $W'$  the adjusted resistance with a positive,  $W''$  that with a negative, and  $f$  the resistance of the testing battery.



deflection is either observed, or (what is better) counteracted by a directing magnet, and the index brought to zero; the key is then raised; the current circulates in both coils of the galvanometer, and any resistance that may be opposed to it in the resistance-coils D. If the position of the needle remains unaltered, the resistance in the coils is equal to the resistance of the line. If it is necessary to use shunts, the result is the same, provided the coils are shunted by equal shunts; if, however, the line offers too great a resistance to be measured by the resistance-coils, we may shunt a greater portion of the current from the coil B of the galvanometer than from the coil A. Say, for instance, we shunt  $\frac{9}{10}$  of the current from A and  $\frac{99}{100}$  from B; then the line-resistance will equal the resistance in the coil multiplied by 10. If the earth-current is not sufficient to give a satisfactory deflection, we have merely to insert a battery of known resistance between the line and galvanometer, and subtract its resistance from the result. It is generally indeed better to do so, as in that case the variations in the earth-current will not be so perceptible.

The differential galvanometer I have designed and constructed for the use of my inspectors is well adapted to this test; and as it has proved so useful in practice, I venture to hope a short description of it may not be unacceptable to the readers of this Magazine.

The exterior form of the instrument is somewhat similar to Varley's original universal galvanometer, and is shown in plan in fig. 3, with the connexions in dotted lines.

The two wires of the coil are wound simultaneously and as carefully parallel as possible, so that their magnetic effects may be equal. Their resistances are also made equal. The needle is suspended by a short fibre of silk from a small bracket in the interior of the coil, which bracket slides in and out in a groove.

When it is desired to render the instrument portable, we can, by turning a milled-head on top of the cover, raise the needle and press it firmly against two pieces of cork on the top of the interior of the coil.

The index is fixed at right angles to the needle. The end of the index plays between the two upright sides of a small frame. This frame is attached to an arm which runs under the base of the instrument, and is moveable in a segment of a circle concentric with the point of suspension of the needle. The bottom of the frame is a small brass plate with a line drawn across it so as to be radial to the circle in which the frame moves. The top of the frame carries two cross wires, which intersect each other in a point exactly over the radial line on the bottom. When a reading is taken, one eye only must be used, and the intersection of

the wires, the end of the index, and the radial line on the bottom of the frame must coincide, so that parallax is avoided. The object of the frame is therefore twofold: first, it serves to limit the vibration of the needle; and, secondly, it gives us a very exact means of noting a deflection. There is a small needle-point attached to the bottom plate, forming a prolongation of the line; and, finally, upon the dial-plate are engraved, instead of a number of degrees or other arbitrary divisions, simply a line at right angles to the coil, to serve as a zero-line, and two radial lines on each side, the use of which we shall presently see.

There is a small socket in the top of the cover of the instrument, immediately over the centre of suspension of the needle, in which may be fixed a rod carrying a directing magnet. In testing ordinary resistance, this magnet may be made to oppose the earth's magnetism, so as to render the needle nearly astatic; or it may be made to counteract a deflection, as in taking battery or line-resistance, as already explained, so as to bring the needle into its most sensitive position; or it may be removed altogether.

Besides all the ordinary tests, to which a differential galvanometer can be applied, we are able with this instrument to measure approximately electromotive force; and for this purpose the radial divisions above referred to are engraved on the dial-plate.

The method is that described by Wheatstone in the *Philosophical Transactions* in 1843. There are, I should have said before, three shunts to each coil, cutting off respectively  $\frac{9}{10}$ ,  $\frac{99}{100}$ , and  $\frac{999}{1000}$  of the current.

In order to determine the position of the two divisions on the left-hand side, I insert the  $\frac{99}{100}$  shunt on each coil; a good ordinary Daniel's cell, such as is commonly used on the line, is then taken and joined up through the galvanometer- and resistance-coils, as shown in fig. 4. Resistance is then inserted until a convenient deflection is obtained; the frame is then made to coincide with the index, and the position of the needle-point marked on the dial-plate; 10 units are then added to the resistance, the deflection is of course reduced, and the frame is made to coincide with the index a second time, and the position of the needle-point again marked on the dial. The two points marked have then conspicuous radial lines engraved through them. On the right-hand side we do exactly the same, except that the unit-cell is formed of an amalgamated zinc plate in 1 of sulphuric acid to 12 of water, and a copper plate in a saturated solution of nitrate of copper; the electromotive force of this element being about 1 volt.

When the electromotive force of a cell or battery has to be measured, we join it up through the galvanometer- and resistance-coils, making the needle deflect either to the right or left,

according as we wish to measure in terms of a volt or of our standard ordinary cell. We then bring the needle-point of the frame to the first of the lines and adjust the resistance until the index coincides with the line and cross wires; we next shift the frame until the needle-point is at the second position, and resistance is added to the circuit until the index agrees a second time with the line and cross wires. The amount of resistance added is a measure of the electromotive force. If the  $\frac{99}{100}$  shunts are used, each unit of the added resistance represents one tenth of the force of the standard cell or the volt, as the case may be. If the  $\frac{999}{1000}$  shunts are used, each unit represents the force of one cell or volt. If the  $\frac{9}{10}$  shunts are used, each unit represents the one hundredth of the force of the standard cell or volt; and, finally, if no shunt at all is used, each unit represents one thousandth of the force of the standard cell or volt. These measurements are quite sufficiently accurate for ordinary use in testing telegraph-batteries, and are quickly and easily taken.

Arconum, January 10, 1872.

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XXI. *Observations on the Corona seen during the Eclipse of December 11th and 12th, 1871.* By G. K. WINTER, *Telegraph Engineer, Madras Railway\**.

[With a Plate.]

MY letter referring to the radial polarization of the corona, published in the Number for January 1870, will doubtless be remembered.

I was again the polariscope-observer in Mr. Pogson's party on behalf of the Madras Government at the eclipse of the 11th and 12th of December last; and as I fear there may be some unavoidable delay in the publication of the official report, I hope the following remarks upon the additional results obtained by myself and others may not be found without value during the discussion of the question.

The instrument I used consisted of a small telescope of 2".75 aperture and about 30" focal length, mounted equatorially. In front of the eyepiece was a polarimeter, consisting of four plates of thin glass mounted in a frame moveable on an axis at right angles to the direction of the bands in a Savart's polariscope, which was fixed in front of the frame; so that the rays from the object-glass passed first through the eyepiece, next through the four plates of glass, and lastly

\* Communicated by the Author.



through the Savart's polariscope to the eye\*. Fig. 1, Plate II. will perhaps render this description clearer.

Although quite convinced myself of the fact of the radial polarization of the corona, I was anxious this time to place it beyond doubt by taking actual measurements of it in such a position that, if the polarized light proceeded from the unobscured portion of the earth reflected into the atmosphere and again back to the eye, it could not be measured. I therefore chose the southern limb for my observations, and carefully got my bands radial to the sun, and consequently making but a small angle with the horizon, before totality, keeping the field as nearly as I could in the same position-angle with respect to the sun by means of the right-ascension tangent-rod during totality. Immediately totality commenced, the white-centred bands appeared. I turned the axis of the frame with the glass plates until the bands disappeared. The angle the plates had to be turned through was  $35^\circ$ . I then turned the declination tangent-screw slightly, so as to get a portion of the corona a small distance from the limb (I think about  $10'$ ) into the field. The plates had then to be turned through an angle of  $45^\circ$  before the bands disappeared. Three other measurements were taken in about the same position, the result showing that the polarization increased considerably with distance from the limb.

It is evident that if the polarized light were really due to the reflection from the unobscured portion of the earth, it would be polarized in a plane nearly at right angles to the plane of my bands, and consequently its polarization could not be neutralized by the plates of glass in the position in which they were used. When the plates were inclined, so as to neutralize the corona polarization, I saw faint black-centred bands on the portion of the moon's disk in the field. I did not observe any when the plates were at right angles to the axis of the telescope; but I think I should have noticed them if they had existed; so that although there was a sensible amount of light on the moon's disk, sufficient to show bands when polarized by the glass plates, I do not think it was perceptibly polarized itself.

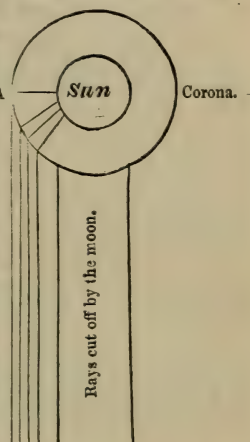
With regard to the evidence of the spectroscope, the existence of the bright line 1474 of Kirchhoff's scale (first observed, I believe, by Mr. Pogson at the eclipse of August 1868) seems fully established. This line†, which is also seen in auroræ, would lead us to suppose that a gas existing in the higher regions of

\* The bands of the polariscope were white-centred when in the plane of polarization.

† Kirchhoff and Ångström, I believe, suppose this line to belong to the spectrum of iron.

the atmosphere, and apparently not met with elsewhere in the earth, forms at all events one of the component parts of the corona. But though we learn, from the fact of the line being bright, that this gas is incandescent, and from its proximity to the sun we should scarcely expect any thing else, yet this fact in no way renders it impossible that much of the light we receive from the corona should be reflected or scattered by minute particles of perhaps denser matter, probably incipient cloud, suspended within it, as such particles are supposed to exist in the earth's atmosphere in order to account for the polarization and blue colour of the sky.

It is well known now that the polarizing-angle of such minute particles is  $45^\circ$ ; and it is evident that a much greater proportion of light reflected at this angle would be received from the portions of the corona at a distance from the sun than from those close to its limb. The annexed woodcut will illustrate my meaning. It is evident that only in case of the ray A, and rays parallel to it, will the angle of incidence be  $45^\circ$ . The light coming from portions of the corona nearer to the limb will evidently consist more and more of light incident at other angles than  $45^\circ$ ; and consequently the proportion of polarized light will be less and less as we approach the sun.



With regard to the photographs of the corona, the boundary seems in all to be well defined; and the streamers, strange to say, do not appear in any of the photographs I have seen. Whatever, therefore, they may be, the light emitted from them does not appear to be nearly so actinic as that received from the corona proper. I have not tested them for polarization, and can therefore say little about them. It seems to me, however, impossible that a gaseous envelope round the sun could possibly take the very irregular form their appearance would indicate.

The glass plates used in the polarimeter were the thin plates used for mounting microscopic objects. I have not yet determined their refractive index; but, supposing them to be of crown-glass and that  $\mu = 1.54$ , then, from the Table given in Professor W. G. Adams's paper in the last March Number of this Magazine, we get the following figures for the proportion of polarized light, as shown by my measurements:—

		Angle of plates.	$\frac{p}{n+p}$ .
1st.	Close to the limb . . . . .	35	·158
2nd.	At about 10' distance from the limb	45	·275
	"                    "	40	·212
	"                    "	45	·275
	"                    "	45	·275
	Mean . . . . .		·239

Arconum, January 27, 1872.

XXII. *Remarks on certain portions of Laplace's Proof of the Method of Least Squares.* By J. W. L. GLAISHER, B.A., F.R.A.S., Fellow of Trinity College, Cambridge\*.

A CONSIDERABLE portion of the fourth chapter of Laplace's *Théorie des Probabilités* is devoted to the investigation of the law of facility of error of the mean &c. of a great number of observations, all the errors of which are subject to the same law of facility  $\phi(x)$ . Laplace, as is well known, obtains his result by the consideration of the coefficient of  $e^{i\varpi i}$  in the expansion of

$$\left\{ \phi\left(\frac{n}{n}\right) e^{-n\varpi i} + \phi\left(\frac{n-1}{n}\right) e^{-(n-1)\varpi i} \dots + \phi\left(\frac{0}{n}\right) \dots + \phi\left(\frac{n}{n}\right) e^{n\varpi i} \right\}^s.$$

A great simplification of this part of the analysis, with increase of generality, was effected by Leslie Ellis, who stated the problem in the following manner†: to find

$$\iint \dots \phi_1(\epsilon_1) \dots \phi_n(\epsilon_n) d\epsilon_1 \dots d\epsilon_n$$

subject to the condition

$$\mu_1 \epsilon_1 + \mu_2 \epsilon_2 \dots + \mu_n \epsilon_n = u.$$

This multiple integral Ellis evaluated approximately by writing for  $\phi_n(\epsilon_n)$  its equivalent  $\phi_n\left(\frac{u - \mu_1 \epsilon_1 \dots - \mu_{n-1} \epsilon_{n-1}}{\mu_n}\right)$ , replacing this last function by the double integral of Fourier's theorem and taking all the integrals between the limits  $\pm \infty$ . Integrating the result, with regard to  $u$ , between  $-l$  and  $l$ , the probability of  $\sum \mu \epsilon$  being intermediate in magnitude to these two quantities is found. The object of the present communication is to obtain the usual result by a method which, though bearing a strong resemblance to Ellis's investigation, nevertheless seems to render the analysis more elegant and symmetrical; the

\* Communicated by the Author.

† Cambridge Philosophical Transactions, vol. viii.



deduction of the law of facility of  $\Sigma\mu\epsilon$ , when  $n$  is made very large, is also presented in a form somewhat different from that of either Laplace or Ellis; and the general formula is verified in several instances by assuming special laws for the individual errors, so as to render the  $n$  integrations capable of accurate performance. The principle made use of, which is due to Lejeune Dirichlet, depends on the discontinuity of the integral

$$\frac{2}{\pi} \int_0^\infty \frac{\sin \theta}{\theta} \cos \gamma \theta d\theta$$

(which = 0 if  $\gamma > 1$ , but = 1 if  $\gamma$  lies between 0 and 1), and may be stated as follows:—Suppose the value of

$$\int \int \dots \phi(x_1, \dots x_n) dx_1 \dots dx_n$$

is required for all values of  $x_1 \dots x_n$ , subject to the condition that  $\psi(x_1, \dots x_n)$  lies between  $\pm 1$ ; then we can replace the multiple integral by

$$\frac{2}{\pi} \int_0^\infty \int_{-\infty}^\infty \int_{-\infty}^\infty \dots \phi(x_1, \dots x_n) \frac{\sin \theta}{\theta} \cos \{ \psi(x_1, \dots x_n) \theta \} d\theta dx_1 \dots dx_n,$$

in which the limits are independent of the variables.

In the case to be considered we require

$$\int \int \dots \phi_1(\epsilon_1) \dots \phi_n(\epsilon_n) d\epsilon_1 \dots d\epsilon_n$$

subject to the condition

$$\mu_1 \epsilon_1 + \mu_2 \epsilon_2 \dots + \mu_n \epsilon_n > -l \text{ and } < l,$$

whence the multiple integral becomes

$$\frac{2}{\pi} \int_0^\infty \int_{-\infty}^\infty \int_{-\infty}^\infty \dots \phi_1(\epsilon_1) \dots \phi_n(\epsilon_n) \frac{\sin \theta}{\theta} \cos \left\{ \theta \left( \frac{\mu_1 \epsilon_1 \dots + \mu_n \epsilon_n}{l} \right) \right\} d\theta d\epsilon_1 \dots d\epsilon_n.$$

Assuming the equal probability of positive and negative errors so that  $\phi_i(\epsilon_i) = \phi_i(-\epsilon_i)$ , then

$$\int_{-\infty}^\infty \phi_i(\epsilon_i) \sin \frac{\mu_i \epsilon_i \theta}{l} d\epsilon_i = 0,$$

whence we have

$$\frac{2}{\pi} \int_0^\infty \left\{ \int_{-\infty}^\infty \phi_1(\epsilon_1) \cos \frac{\mu_1 \epsilon_1 \theta}{l} d\epsilon_1 \right\} \dots \left\{ \int_{-\infty}^\infty \phi_n(\epsilon_n) \cos \frac{\mu_n \epsilon_n \theta}{l} d\epsilon_n \right\} \frac{\sin \theta}{\theta} d\theta. \quad \dots \quad (1)$$

Now

$$\int_{-\infty}^{\infty} \phi_i(\epsilon_i) \cos \frac{\mu_i \epsilon_i \theta}{l} d\epsilon_i = \int_{-\infty}^{\infty} \phi_i(\epsilon_i) \left(1 - \frac{\mu_i^2 \epsilon_i^2 \theta^2}{2l^2} + \dots\right) d\epsilon_i \\ = 1 - \frac{\mu_i^2 k_i^2}{l^2} \theta^2 + \dots,$$

putting  $k_i^2$  for  $\int_0 \phi_i(\epsilon_i) \epsilon_i^2 d\epsilon_i$ ; the first term is unity, since

$$\int_{-\infty}^{\infty} \phi(\epsilon_i) d\epsilon_i = 1. \text{ Thus (1) becomes}$$

$$\frac{2}{\pi} \int_0^{\infty} e^{\Sigma \log(1 - h_i^2 \theta^2 + \dots)} \frac{\sin \theta}{\theta} d\theta = \frac{2}{\pi} \int_0^{\infty} e^{-\Sigma h_i^2 \theta^2 - A\theta^4 - B\theta^6 - \dots} \frac{\sin \theta}{\theta} d\theta,$$

$h_i^2$  being written for  $\frac{\mu_i^2 k_i^2}{l^2}$ . Now,  $n$  being very large,  $\Sigma h_i^2 \theta^2$  is of the order  $n\theta^2 = an\theta^2$ , say; similarly  $A, B, \&c.$  are of the same order; so that the integral takes the form

$$\frac{2}{\pi} \int_0^{\infty} e^{-an\theta^2 - bn\theta^4 - cn\theta^6 - \dots} \frac{\sin \theta}{\theta} d\theta.$$

Since  $n$  is very large, the exponential is finite only when  $\theta$  is very small and of the order  $n^{-\frac{1}{2}}$ , so as to make  $n\theta^2$  finite; when this is the case, the other terms are of the orders  $\theta^2, \theta^4, \&c.$ , and may be neglected. It is to be observed that if  $\theta$  is of the order  $n^{-\frac{1}{4}}$ , so as to make  $n\theta^4$  finite, the first term is of the order  $\theta^{-2}$ , and the value of the exponential is infinitesimal. We may therefore neglect all the terms except the first, so that the integral becomes

$$\frac{2}{\pi} \int_0^{\infty} e^{-an\theta^2} \frac{\sin \theta}{\theta} d\theta = \frac{2}{\sqrt{\pi}} \operatorname{Erfc} \frac{1}{2\sqrt{na}} \\ = \frac{2}{\sqrt{\pi}} \operatorname{Erfc} \frac{l}{2\sqrt{\Sigma \mu_i^2 k_i^2}},$$

which is the well-known result. The integral made use of, viz.

$$\int_0^{\infty} e^{-ax^2} \frac{\sin bx}{x} dx = \sqrt{\pi} \int_0^{\frac{b}{2\sqrt{a}}} e^{-u^2} du = \sqrt{\pi} \operatorname{Erfc} \frac{b}{2\sqrt{a}},$$

is obtained at once by the integration of

$$\int_0^{\infty} e^{-ax^2} \cos bxdx = \frac{\sqrt{\pi}}{2\sqrt{a}} e^{-\frac{b^2}{4a}}$$

with regard to  $b^*$ .

\* See Phil. Mag. vol. xlii. pp. 298 and 421 (October and December 1871).

The reasoning that occurs near the conclusion of the above investigation may also be exhibited in another form, which is perhaps clearer.

Resuming equation (1), we may write it

$$\frac{2}{\pi} \int_0^\infty \frac{\sin \theta}{\theta} \{1 - h_1^2 \theta^2 + \dots\} \dots \{1 - h_n^2 \theta^2 + \dots\} d\theta.$$

Put  $\theta = \frac{\theta'}{\sqrt{\sum h_i^2}}$ , and this becomes

$$\begin{aligned} & \frac{2}{\pi} \int_0^\infty \frac{\sin \frac{\theta'}{\sqrt{\sum h_i^2}}}{\frac{\theta'}{\sqrt{\sum h_i^2}}} \left\{ 1 - \frac{h_1^2 \theta'^2}{\sum h_i^2} + \dots \right\} \dots \left\{ 1 - \frac{h_n^2 \theta'^2}{\sum h_i^2} + \dots \right\} d\theta' \\ &= \frac{2}{\pi} \int_0^\infty \sin \frac{\theta'}{\sqrt{\sum h_i^2}} \left\{ 1 - \frac{\theta'^2}{\sum h_i^2} + \dots \right\}^{h_1^2} \dots \left\{ 1 - \frac{\theta'^2}{\sum h_i^2} + \dots \right\}^{h_n^2} \frac{d\theta'}{\theta'} \\ &= \frac{2}{\pi} \int_0^\infty \sin \frac{\theta'}{\sqrt{\sum h_i^2}} \left\{ 1 - \frac{\theta'^2}{\sum h_i^2} + \dots \right\}^{\sum h_i^2} \frac{d\theta'}{\theta'} \\ &= \frac{2}{\pi} \int_0^\infty \sin \frac{\theta'}{\sqrt{\sum h_i^2}} e^{-\theta'^2} \frac{d\theta'}{\theta'}, \end{aligned}$$

as before. The legitimacy of the neglect of the terms beyond  $\theta^2$  is best seen by taking the simpler case of  $\phi_1 = \phi_2 = \&c.$ ,  $\mu_1 = \mu_2 = \&c.$ ; the expression is then of the form

$$\left( 1 - \frac{h^2 \theta^2}{n} + \frac{A \theta^4}{n^2} - \dots \right)^n, \quad . \quad . \quad . \quad (2)$$

the limit of which clearly is  $e^{-h^2 \theta^2}$ . It is to be remarked that the fact of  $\int_0^\infty \theta^{2n} \frac{\sin \theta}{\theta} d\theta$  being infinite in no way prejudices the reasoning, as we are concerned with the series

$$\int_0^\infty \frac{\sin \theta}{\theta} (1 - A \theta^2 + B \theta^4 \dots) d\theta$$

as a whole; and the function in brackets must always be less than unity, since it is the product of  $n$  factors of the form

$\int_0^\infty \phi(\epsilon_i) \cos \alpha \epsilon_i d\epsilon_i$ ; and this,  $\phi_i(\epsilon_i)$  being always positive, is less than  $\int_{-\infty}^\infty \phi(\epsilon_i) d\epsilon_i$  (that is, than unity).

It is worth while to examine rather more carefully the approach



to the limit as  $n$  increases, since (2) has  $e^{-h^2\theta^2}$  for its limit only when  $\frac{\theta^2}{n}$  is of the order  $\frac{1}{n}$ , and the limits of the integration are from  $\theta=0$  to  $\theta=\infty$ . The quantity to be considered is

$$\int_0^\infty \frac{\sin \theta}{\theta} (1 - h^2\theta^2 + A\theta^4 - \dots)^n d\theta, \quad \dots \quad (3)$$

which we may write

$$\begin{aligned} \int_0^{\frac{\alpha}{\sqrt{n}}} \frac{\sin \theta}{\theta} (1 - h^2\theta^2 + \dots)^n d\theta \\ + \int_{\frac{\alpha}{\sqrt{n}}}^\infty \frac{\sin \theta}{\theta} \left\{ \int_{-\infty}^\infty \phi(\epsilon) \cos \frac{\mu\epsilon\theta}{l} \right\}^n d\theta, \quad \dots \quad (4) \end{aligned}$$

where  $\alpha$  is a finite quantity: the first of these integrals is equal to  $\int_0^\alpha \sin \frac{\theta}{\sqrt{n}} e^{-h^2\theta^2} \frac{d\theta}{\theta}$ , which differs from  $\text{Erfc}\left(\frac{1}{2h\sqrt{n}}\right)$  by

$$\int_\alpha^\infty \sin \frac{\theta}{\sqrt{n}} e^{-h^2\theta^2} \frac{d\theta}{\theta}.$$

We can see in a general way that this integral must be much smaller than the previous one, owing to the rapid decrease of the exponential factor. Also, as  $\alpha$  is at our disposal, subject only to the condition that  $\frac{\alpha}{\sqrt{n}}$  must be of the order  $\frac{1}{\sqrt{n}}$ , we can by taking it large render the latter integral very small indeed. With regard to the second integral in (4), since  $\theta$  is always greater than  $\frac{\alpha}{\sqrt{n}}$ ,  $\int_{-\infty}^\infty \phi(\epsilon) \cos \frac{\mu\epsilon\theta}{l} d\epsilon$  must differ in defect from  $\int_{-\infty}^\infty \phi(\epsilon) d\epsilon (=1)$  by a finite quantity; so that the integral is less than  $\frac{\pi}{2}\beta^n (\beta < 1)$ , which may be neglected compared with  $\text{Erf}\left(\frac{1}{2h\sqrt{n}}\right)$ .

I have been unsuccessful in several attempts to prove rigorously the perfect legitimacy of replacing (3) by  $\text{Erfc}\left(\frac{1}{2h\sqrt{n}}\right)$  when  $n$  is large. There appears to be a real difficulty inherent in this portion of the reasoning, as similar ambiguities present themselves at the corresponding points in Laplace's and Ellis's investigation. There would be no difficulty if we might take  $n$  to be an infinity of a superior grade to the infinite limit of the in-

tegral, so as to replace  $\left(1 - \frac{h^2\theta^2}{n} + \dots\right)^n$  by  $e^{-h^2\theta^2}$  throughout the whole extent of the integration with regard to  $\theta$ ; but practically  $n$  is merely a large finite number. It is probable that a perfectly general and rigorous mathematical demonstration of the law of facility cannot be given on Laplace's principles; but, at all events, the precise nature of the assumptions necessary for its truth might be investigated with advantage. This could probably be best effected by careful examination of one or two special cases; but in those that follow I shall make no attempt to examine the point just noticed very carefully; it will only be shown that, admitting it, the law is verified. To simplify the expressions, the laws of facility  $\phi_1, \phi_2, \&c.$  will all be supposed the same, and  $\mu_1, \mu_2, \&c.$  will be taken each equal to unity.

Taking first the case discussed by Leslie Ellis, viz. when  $\phi(\epsilon) = \frac{1}{2}e^{\mp\epsilon}$ , the lower sign being taken when  $\epsilon$  is negative, the integral (1) becomes

$$\begin{aligned} & \frac{2}{\pi} \int_0^\infty \left\{ \int_0^\infty e^{-\epsilon} \cos \frac{\epsilon\theta}{l} d\epsilon \right\}^n \frac{\sin \theta}{\theta} d\theta \\ &= \frac{2}{\pi} \int_0^\infty \sin \frac{\theta}{\sqrt{n}} \left(1 + \frac{\theta^2}{nl^2}\right)^{-n} d\theta \\ &= \frac{2}{\pi} \int_0^\infty \sin \frac{\theta}{\sqrt{n}} e^{-\frac{\theta^2}{l^2}} \frac{d\theta}{\theta} = \frac{2}{\sqrt{\pi}} \operatorname{Erfc} \left( \frac{l}{2\sqrt{n}} \right), \end{aligned}$$

on the supposition that we may in the last two integrals imagine the infinite limit replaced by a finite quantity when we please, or that we may imagine  $n$  increased absolutely *sine limite*. This investigation is very much shorter than Ellis's, which is obtained by expanding the circular function in ascending powers of  $\theta^*$ . It might for the moment appear as though, since positive and negative errors are equally likely, the law of facility  $\phi(\epsilon)$  must be a function of  $\epsilon^2$ . It is clear, however, that such a law as that taken above, viz.  $e^{-\sqrt{\epsilon^2}}$ , is quite as admissible; there is no reason why the algebraical expression for the law should be continuous.

There are not a great many forms of  $\phi$  for which  $\phi(\epsilon) \cos a\epsilon d\epsilon$  can be integrated between the limits 0 and  $\infty$  in finite terms. The following two cases, however, are instances of such forms:

\* Camb. Trans. vol. viii. p. 213. In the integral there discussed  $\cos \theta$  takes the place of  $\frac{\sin \theta}{\theta}$ ; the reasoning, however, is not affected thereby.

Ellis assumes  $n$  to be so great that it is greater than the number of terms in the series for  $\cos \theta$ ; i. e. he takes it to be an absolute infinity.

$$\int_{-\infty}^{\infty} \frac{\cos bx}{e^{ax} + e^{-ax}} dx = \frac{\pi}{a} \frac{1}{e^{\frac{b\pi}{2a}} + e^{-\frac{b\pi}{2a}}}, \dots \dots \dots (5)$$

$$\int_{-\infty}^{\infty} \frac{e^{-c^2 x^2} \cos bx}{a^2 + x^2} dx = \frac{\sqrt{\pi}}{a} e^{a^2 c^2} \left\{ e^{-ab} \operatorname{Erf} \left( ac - \frac{b}{2c} \right) + e^{ab} \operatorname{Erf} \left( ac + \frac{b}{2c} \right) \right\}^* (6)$$

Let us take, therefore,  $\phi(\epsilon) = \frac{2a}{\pi} \frac{1}{e^{a\epsilon} + e^{-a\epsilon}}$ , and the resulting integral is

$$\begin{aligned} & \frac{2}{\pi} \int_0^{\infty} \left( \frac{2}{e^{\frac{\pi\theta}{2al}} + e^{-\frac{\pi\theta}{2al}}} \right)^n \frac{\sin \theta}{\theta} d\theta \\ &= \frac{2}{\pi} \int_0^{\infty} \left( 1 + \frac{\pi^2 \theta^2}{8a^2 l^2} + \dots \right)^{-n} \frac{\sin \theta}{\theta} d\theta = \frac{2}{\sqrt{\pi}} \operatorname{Erfc} \left( \frac{\sqrt{2} al}{\pi \sqrt{n}} \right). \end{aligned}$$

Similarly, if, in accordance with (6), we take as our law

$$\phi(\epsilon) = \frac{a}{2\sqrt{\pi} e^{a^2 c^2} \operatorname{Erf} ac} \frac{e^{-a^2 \epsilon^2}}{a^2 + \epsilon^2},$$

the resulting integral

$$= \frac{2}{\pi} \int_0^{\infty} \left( 1 - \frac{ae^{-a^2 c^2} \theta^2}{2l^2 c \operatorname{Erf} ac} + \dots \right)^n \frac{\sin \theta}{\theta} d\theta = \frac{2}{\sqrt{\pi}} \operatorname{Erfc} \left\{ \frac{l\sqrt{c} \operatorname{Erf} ac}{e^{-\frac{1}{2} a^2 c^2} \sqrt{2an}} \right\}.$$

Poisson † has demonstrated that, if  $\phi(\epsilon) = \frac{1}{\pi} \frac{1}{1 + \epsilon^2}$ , the law of facility is not of the form  $e^{-h^2 \epsilon^2}$ . This arises from the discontinuity in the value of  $\int_0^{\infty} \frac{\cos a\epsilon}{1 + \epsilon^2}$ , which  $= \frac{\pi}{2} e^{\mp a}$ . It can be easily shown that the same discontinuity occurs if  $\phi(\epsilon)$  be proportional to  $\frac{1}{a^4 + \epsilon^4}$  &c. This virtually includes any rational algebraical law; for  $\phi(\epsilon)$  must not be infinite, either when  $\epsilon$  is finite or infinite; so that the most general expression is

$$\phi(\epsilon) = \Sigma \frac{A}{\epsilon^2 + a^2} + \Sigma \frac{B}{\epsilon^4 + a} + \&c.;$$

thus the law of facility would not be proportional to  $e^{-h^2 \epsilon^2}$  if the facilities of the individual errors were rational algebraical functions.

With reference to the proofs of the law of facility that have

\* *Phil. Mag. loc. cit.* p. 298.

† *Connaissance des Temps*, 1827.



been given, it appears that Laplace's and Gauss's second demonstration depend on principles the general truth of which is apparent on consideration, and the mathematical difficulties are not of a very serious kind. The illustration derived from the deflection of a stone let fall on to a plane, first given by Sir John Herschel in the *Edinburgh Review*, has been sometimes regarded as a proof of the law\*. So far from being self-evident, it appears to me that the assumption there made of independent  $x$  and  $y$  deflections is one of the most striking consequences of Laplace's law of facility. *A priori*, one would be inclined to think that the largeness of an  $x$  deflection would increase the probability of a small  $y$  deflection, as then the total deflection would be rendered less. At all events, the independence of the  $x$  and  $y$  deflections is a very unexpected result when it is considered that it may be derived as a consequence of the sole assumption that errors that occur are due to the aggregation of errors arising from a great number of sources. With reference to Herschel's problem, Ellis has remarked that "there is no shadow of reason for supposing that the occurrence of a deviation in one direction is independent of that in another, whether the two directions are at right angles or not" (*Phil. Mag.* vol. xxxvii. (1850) p. 325).

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XXIII. *On Resonance, and on the Circumstances under which Change of Phase accompanies Reflection.* By ROBERT MOON, M.A., Honorary Fellow of Queen's College, Cambridge†.

IN a former paper‡ I pointed out the misapprehensions which have arisen with regard to the resonance which occurs when a disk or tuning-fork is made to vibrate near the open end of a tube of which the other extremity is closed, and whose length is equal to one *quarter* of the length of a wave having the same periodic time as the disk or fork; and I showed that the augmentation of sound which under those circumstances results is due to the fact that the ærial disturbance caused by each movement in either direction of the vibrating body, by reflection at the closed end of the tube, is brought to bear on the disk or fork during its movement in the opposite direction, so as to increase the amplitude of its vibration.

If the disk and tuning-fork are replaced by a bell which can be made to vibrate by means of a bow drawn across its rim, and the width of the tube is at the same time enlarged, its length continuing equal to one quarter of the length of a wave of the

\* As in Thomson and Tait's '*Natural Philosophy*,' vol. i. p. 314.

† Communicated by the Author.

‡ "On a Simple Case of Resonance," inserted in the *Philosophical Magazine* for February.

same periodic time as the vibrating body, effects similar in character but still more prominent and striking will occur (Tyndall 'On Sound,' p. 176).

If we further substitute for the pipe stopped at one end an open tube twice the length of the former, like results will continue to be perceptible.

The method of explanation I have already developed applies in principle to this latter case also.

For, suppose the first wave propagated by the bell down the tube to be a condensation. At the moment when the front of the wave reaches the aperture, a reflection will begin to take place from the latter; the reflected wave, as I shall presently show, being a wave of *rarefaction*, and not a wave of condensation, as would have been the case had the reflection taken place from a fixed obstacle. Such being the case, if we represent by  $2t$  the period of vibration, going and returning, of the bell, we shall have within the tube at the end of  $2t$  a rarefied wave travelling *from* the aperture and completely filling the tube. Hence, during the *third* interval  $t$  (*i. e.* while the bell is moving for the second time *towards* the tube), the rarefied wave reflected back from the aperture in manner already explained will be undergoing a second reflection at the bell, and so will bring to bear upon the latter a rarefaction which must have the effect of accelerating its motion; and so for each successive demi-vibration of the bell.

The fact that a wave of condensation after traversing a tube will send back a wave of rarefaction when it arrives at the open extremity of the tube, may be proved either popularly or analytically. As the point is of considerable importance I shall here pursue the latter method.

Adopting the notation of the *Encyclopædia Metropolitana*, any disturbance of the air within a cylindrical tube may either be represented by one or other of the two following systems of equations; viz.

$$\left. \begin{array}{l} \text{velocity} \quad . \quad . \quad = \phi(at-x), \\ \text{condensation} \quad = \frac{\phi(at-x)}{a}, \end{array} \right\} . . . . (1)$$

or

$$\left. \begin{array}{l} \text{velocity} \quad . \quad . \quad = \psi(at+x), \\ \text{condensation} = -\frac{\psi(at+x)}{a}, \end{array} \right\} . . . . (2)$$

or else will be capable of resolution into two disturbances—one of which, represented by (1), will be propagated to the right if  $x$  be measured positively in that direction, while the other, re-

presented by (2), will be propagated to the left\*: and in this case the combined disturbance will be represented by the system

$$\left. \begin{aligned} \text{velocity} \quad . \quad &= \phi(at-x) + \psi(at+x), \\ \text{condensation} &= \frac{\phi(at-x) - \psi(at+x)}{a}. \end{aligned} \right\} \quad . \quad (3)$$

If the tube be supposed to lie on the right of the bell, it is clear that the wave of condensation which the first movement of the bell tends to propagate within the tube may be represented before it reaches the further extremity of the tube by the system

$$\left. \begin{aligned} \text{velocity} \quad . \quad &= f(at-x), \\ \text{condensation} &= \frac{f(at-x)}{a}, \end{aligned} \right\} \quad . \quad . \quad . \quad (4)$$

As this wave emerges from the tube, the condensation of any thin stratum of air which occupies the plane of the aperture will be *diminished*; i. e., by reason of its being in contact with the free atmosphere outside the tube, its condensation will be less than it would have been had the tube been prolonged indefinitely beyond the aperture. Moreover such diminution of condensation of the air occupying the aperture will necessarily be accompanied by an *increase* of its velocity, since, the direction of particle-motion in a wave of condensation being always the same as that of propagation, a diminution of density on the side towards which the motion takes place must lead to an acceleration of the latter.

Hence, measuring  $x$  from the aperture, instead of the velocity and condensation at the aperture being represented by

$$\left. \begin{aligned} \text{velocity} \quad . \quad &= f(at), \\ \text{condensation} &= \frac{f(at)}{a}, \end{aligned} \right\}$$

as they would be if the tube were prolonged indefinitely beyond the aperture, they must be of the form indicated by the system

$$\left. \begin{aligned} \text{velocity} \quad . \quad &= f(at) + f_1(at), \\ \text{condensation} &= \frac{f(at) - f_2(at)}{a}; \end{aligned} \right\} \quad . \quad . \quad . \quad (5)$$

where, assuming the velocity to be measured positively in the

\* See *Encyc. Met.* art Sound, No. 128. It may be remarked that although the equations in the text do not apply to what takes place outside the tube as the wave emerges from it, they do apply to what takes place within the tube, which is sufficient for our purpose.



same direction in which  $x$  is measured positively, we have  $f, f_1$  and  $f_2$  all positive.

But, from what has preceded, it results that the disturbance in the plane of the aperture must be represented by systems of equations of one or other of the three following forms, viz. :—

$$\left. \begin{array}{l} \text{velocity} \quad . \quad . = \phi(at), \\ \text{condensation} = \frac{\phi(at)}{a}; \end{array} \right\} . . . . . (6)$$

$$\left. \begin{array}{l} \text{velocity} \quad . \quad . = \psi(at), \\ \text{condensation} = -\frac{\psi(at)}{a}; \end{array} \right\} . . . . . (7)$$

$$\left. \begin{array}{l} \text{velocity} \quad . \quad . = \phi(at) + \psi(at), \\ \text{condensation} = \frac{\phi(at) - \psi(at)}{a}; \end{array} \right\} . . . . . (8)$$

and it is clear that no values which can be assigned to  $f_1, f_2$  will reduce (5) to the form of (6) or (7). It must therefore become identical in form with (8), in order to which we must have  $f_2 = f_1$ ; whence it is clear that the disturbance within the tube during the period in which the original wave of condensation is endeavouring to escape from the aperture is resolvable into two disturbances, one of which, propagated to the right, is identical with the original condensation represented by (4), while the other will be propagated to the left, and will be represented by

$$\begin{array}{l} \text{velocity} \quad . \quad . = f_1(at+x), \\ \text{condensation} = -\frac{f_1(at+x)}{a}, \end{array}$$

and therefore, the expression for the condensation being negative, will be a *rarefaction*.

In precisely the same manner we might prove that a wave of rarefaction, after traversing a tube open at both ends, when finally emerging from the tube will send back a *condensation*.

In like manner also it may be shown that when two gases touch each other along a given plane without intermingling, and a pulse is transmitted through the one to the other in a direction normal to the plane of contact, the wave reflected from the latter will be of the same phase as the incident wave, or the opposite phase, according as the density of the second gas is greater or less than that of the first.

The same holds when a disturbance is propagated along a stretched cord consisting of two pieces of unequal density.

It may in fact be stated as a general truth, that whenever a

wave is reflected under such circumstances as to make the particle-velocity at the point of reflection *greater* than it would be if the wave continued to be transmitted through a uniform medium, the incident and reflected waves will be of opposite phases; and for this reason, viz. that the directions of transmission of these waves being opposite, when they are in opposite phases their particle-velocities will *coincide* in direction, and *vice versâ*.

The bearing of this principle upon the undulatory theory of Newton's rings will not fail to be adverted to.

6 New Square, Lincoln's Inn,  
February 5, 1872.

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XXIV. *On the Action of Nuclei in separating Gas or Vapour from its Supersaturated Solution.* By CHARLES TOMLINSON, F.R.S.\*

IN the Philosophical Magazine for August and September 1867, and in the Proceedings of the Royal Society for 1868-69 (p. 248), are two papers by me, on the Action of Nuclei in separating Gas from Soda-water &c., and Vapour from Liquids at or near the boiling-point. In compliance with a request of the Abbé Moigno, I furnished him with an account of these two papers for insertion in *Les Mondes*, in which they appeared in the Numbers for the 12th of October and the 2nd of November last. In the Number for the 21st of December last, the Rev. P. Sanna Solaro, S.J., has made some critical remarks on these two papers, in which I endeavour to trace the action of nuclei in separating gas and vapour from their supersaturated solutions, although the remarks in question are chiefly confined to the separation of vapour.

I may remark that the term "gaseous supersaturated solution" refers to such liquids as soda-water, Seltzer water, and champagne; and seeing that, in a large number of cases in which nuclei separated gas from them, there was a precisely similar action of nuclei in separating vapour from liquids at or near the boiling-point, it seemed not unreasonable to suppose that these last-named liquids are constituted like the former. Moreover in both cases the received opinion is that nuclei act by carrying down air, into which the gas or the vapour is said to expand, and so escape; whereas, according to my theory, I endeavour to show that as gas or vapour will adhere to an oily, fatty, or greasy body, or to a body that has been handled, while water will not so adhere, it is only necessary to introduce such a body into the solution to see that it becomes immediately covered with bubbles of gas or of vapour. These bubbles escape from its sur-

\* Communicated by the Author.

face so long as it continues to be covered, more or less, with a film of a body that can be touched by air or vapour and not by water. A flint pebble that has been exposed to the air of a room or handled, and put into a solution of gas or of vapour, immediately becomes covered with bubbles; but if broken in half and returned to the solution, not a single bubble is to be seen on the fractured parts; for these are specimens of nature's clean surfaces. If air has any function to perform in the matter, why should the unclean, and not the clean surfaces carry it down?

Some liquids contain their own nuclei, as in the case of milk. When this is heated over the fire, it becomes more and more charged with vapour; and at a certain point, the particles of butter disseminated through it, assisting the expansive force of the heat, produce such a sudden burst of vapour as to cause the liquid to boil over.

If a body, such as a glass rod, be made chemically clean, and then be plunged into a supersaturated solution of gas or of vapour, not a single bubble will be seen upon it, because both water and gas, or water and vapour, adhere to it with equal force. If the clean, but wet, glass rod be left to dry in the dusty air of the room, and when dry be plunged into the solution, it will be active; but if left to dry in the pure outer air of the country, and when dry be plunged into the solution, it is inactive, because it is still in a clean or catharized state. The same remark applies to a supersaturated saline solution.

I find it difficult to reply to such objections as those that M. Solaro has brought forward, because, apparently without repeating my experiments, he sometimes refuses to accept my account of them as true. For example, he cannot understand how a cage of fine wire gauze can be introduced into a liquid at or near the boiling-point and still retain some of its air. "*La cage était descendue doucement dans l'eau, et, par conséquent, à mesure que sa partie inférieure descendait, l'air devait sortir doucement aussi par la partie supérieure.*" If, instead of making this remark, M. Solaro had simply tried the experiment, he would not have placed himself in the position of the man who, being told that the facts were against him, replied, "So much the worse for the facts!"

Again, when I produce ebullition by the contact of an unclean body with a liquid near its boiling-point, M. Solaro says "*P'ébullition arriverait sans le contact quelques instants plus tard.*" Now that is just the very thing that it would not do; for I arranged the hot-water bath so as to keep the liquid to be operated on near to, but not at the boiling-point.

Once more, I say that the glass vessels in which liquids are usually boiled and distilled by the chemist, are frequently dotted



over with minute points of carbon &c., which act as excellent nuclei and save many a vessel from destruction. M. Solaro makes me say that these are points which the eye cannot detect; whereas I say no such thing. Any one who boils liquids in glass vessels must have noticed ascending vortices of vapour from certain points in the glass, and that these points consist of small black specks of carbon which act as excellent nuclei. Hence it cannot be said of these, "lorsque tout l'air ou le gaz a disparu, le phénomène a cessé, mais seulement alors et par cette raison." Any one of these points will remain active during many hours, and surely it cannot be seriously maintained that during all this time it is giving off air.

M. Solaro asks, "Pouvons-nous supposer qu'un corps quelconque se purifie par le simple contact d'un liquide chaud quel qu'il soit?" The action of liquids at the boiling-point in rendering bodies chemically clean ought to be well known to every one who has distilled sulphuric acid or alcoholic or ethereal liquids, were it not that the idea is so strongly fixed in the mind that all "promoters of vaporization" act by carrying down air. Sand, cleaned by boiling in sulphuric acid, washing, and heating, or clean mercury (as described in my experiments) introduced into boiling water, must surely, according to the theory advocated by M. Solaro, carry down air; but they produce *soubresauts* at once; whereas a little unclean sand or unclean mercury stops the *soubresauts* and produces tranquil boiling.

The permanent nuclei, such as charcoal and other porous bodies, act, as I believe, on Saussure's principle of the absorption of gases and vapours. A piece of charcoal will act for many hours, and even days, in liberating vapour from a boiling liquid; and can it be supposed for a moment that during all this time it is giving off air as well as vapour?

If there are any other points in M. Solaro's objections that I have not noticed, it is because I am unwilling to repeat what has been already said in my two papers. In the second paper I have pointed out the different results obtained as to temperature by heating a liquid by means of a flame applied below, and by placing a vessel in a bath of hot oil or other source of heat. I have also shown how very improbable it is that gases such as nitrogen, which are so little soluble in water, especially in boiling water, should continue to exist in it after long boiling. I have often repeated Mr. Grove's experiment, in which water covered with oil was repeatedly boiled. I covered the water with paraffine-oil and boiled it several times a day during a week. The result was, that, if the boiling be somewhat brisk, the oil becomes broken up into globules, which are carried down to the bottom of the tube. If less brisk, the surface of the oil opens, lets in air and closes upon it; the air then

risers to the surface in bubbles. In this way ten or twelve bubbles of air may form a ring on the surface in contact with the glass. If a small flame of a spirit-lamp be now applied to the bottom of the tube, a bubble of steam will often ascend through the oil and fish down one or two or three of these bubbles, which may burst and scatter a multitude of minute bubbles against the side of the tube, where they remain a long time. It would be quite natural to suppose that these bubbles are derived from the water instead of from the superincumbent air. It is curious to see how easily a bubble of steam adheres to a bubble of air. I believe this to be the source of the air found in boiling liquids in the form in which this experiment is arranged.

In conclusion, I will give the results of a few experiments which were made after reading M. Solaro's paper.

*Exp. 1.* Distilled water was poured into a clean flask, and this was placed over the flame of a spirit-lamp. Two vortices of steam-bubbles continued to ascend from two small but visible black specks in the bottom of the flask so long as the boiling was continued.

*Exp. 2.* A rat's-tail file placed in boiling water gave off steam from every part of the immersed surface. The file was washed in soap and water. It was now active at several points. It was then washed in alcohol, it was still active, and on closely examining these points they were evidently rust. The file was put into dilute sulphuric acid and dried in hot air. It was then inactive. Now let me ask why the file, after the first cleaning, should act in one or two points only, and not over its whole surface, if, according to the theory, rough bodies are most effective in carrying down air?

*Exp. 3.* Drew the file through the hand that had been lightly smeared with oil. The file when introduced into the hot water was immediately covered with bubbles, and bubbles escaped from its surface during several minutes, so long as the water was just about the boiling-point.

*Exp. 4.* A clean glass rod put into boiling water was entirely free from bubbles. It was then covered with a film of castor-oil, and when restored to the boiling water became immediately covered with small bubbles, while large bubbles escaped from various parts of its surface.

*Exp. 5.* Wood-spirit, boiling at  $140^{\circ}$  F., contained in a test-tube, was plunged into a flask of hot water. A clean glass rod that had been exposed during an hour to the air of my garden was inactive. It was drawn through the hand that had been made slightly greasy with lard, and when reinserted it produced such a burst of vapour as to turn out half the contents of the tube.

A similar result was produced with ether and bisulphide of carbon.

Highgate N., February 12, 1872.

XXV. *The Origin of Malaria.* By DANIEL VAUGHAN\*.*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

Cincinnati, December 30, 1871.

I SHOULD like to submit to scientific men a brief exposition of my researches on Malaria, as I think they will be received with some degree of interest. The present communication contains the chief points given in two papers at the Academy of Medicine of this city; but it is prepared with more care and better adapted to the character of the *Philosophical Magazine*. Hoping that you will give it publication at your earliest convenience,

I remain,

Yours very truly,

DANIEL VAUGHAN.

While excessively moist lands in warm climates are known to be very unfavourable to health, no adequate cause for the evils is to be found in the local variation in the gaseous constituents of the air, or in any direct influence of the aqueous vapour which it contains. To remove the mysteries connected with the insalubrity of such localities, the course of experimental inquiry has recently been directed to a search for the organic matter in air and in water; but an examination of the sources from which this matter is derived may contribute much to reveal its peculiar characters and its effects on the human frame. For the greater portion of the carbonaceous matter which it contains the atmosphere is indebted to the vegetable kingdom; and to this great repository of carbon we may first look for the source of marsh-poison, which is generally regarded as having a vegetable origin.

Though serious evils may be justly imputed to the dust which occasionally floats around us, they must be excluded from consideration in inquiries respecting the insalubrity of marshes. In these permanent abodes of moisture dust cannot be expected to rise from the ground; and its solid particles would be only removed from the air by those rains which observation proves to be instrumental in developing the activity of malaria. Alkaloids, vegetable acids, and the salts which they form might mingle with the fresh waters of our lands; yet they cannot be expected to pass into the air in a quantity sufficient to affect in a serious degree the economy of animal life. But the volatile oils emitted by living and decaying plants have the greatest tendency to charge the air with their vapours; and as the extent of this impregnation depends chiefly on heat, they appear

\* Communicated by the Author.



to correspond to those malarious emanations which marshes are ready to send forth in obedience to the elevation of temperature.

From the limited scale on which they are produced on our globe, these volatile oils could have little influence on the condition of human health if they were uniformly diffused through the atmosphere, or even if they entered it by vegetable exhalation alone. But, from their slight solubility in water, they are collected during rains into low marshes ; and under the influence of heat the air in these localities must be filled with the vapours of the volatile organic matter produced on the high lands around them. The course of nature in this case is very similar to that exhibited on a small scale in obtaining volatile oils or perfumes from different parts of plants by aqueous distillation. On distilling with water the vegetable products in which they are contained, the essential oils pass over with the steam and are obtained by condensation in a pure or in a concentrated form. Though it is not always necessary that they should dissolve in the fluid before volatilizing, yet the solution generally takes place ; and it contributes to the success of the operation in cases where the essential oils form but a small part of the materials from which they are manufactured. In obtaining the ferment oils, the leaves and other parts of plants capable of affording them require to be steeped in water for some days ; and distillation is not performed until the odour of the fluid gives indication that the oils have been generated. The mode generally adopted for obtaining these volatile principles from vegetable matter depends on their slight solubility in water, and on the manner in which they are affected by an increase of temperature.

From these results it is easy to trace the inevitable course of similar operations transpiring in nature on a far more extensive scale. Of the rain which falls during warm seasons, a large part comes into repeated contact with numerous vegetable forms as it descends along the leaves and branches of trees or moves through the flowers and herbage of verdant fields ; so that on reaching the valleys the water must be contaminated with the essential oils of living plants, and with the ferment oils of their decaying parts. In this manner a marsh, an enclosed lake, or a pond becomes the receptacle of a large portion of the volatile organic matter generated within the basin from which its waters are collected. The volatile oils which, on the occurrence of cool and heavy rains, are thus concentrated from a wide area into a marsh, change into vapour as the water becomes warm ; and they will be most ready to contaminate the air over such permanent abodes of moisture during the intense heat of summer.

The relation which I have traced between the processes of

nature and of art for concentrating into a limited space the diffusible oils of plants, does not require that the water of a marsh should be subjected to a boiling heat. This will appear evident from well-known facts and principles respecting vaporization; and without insisting on the rigorous accuracy of Dalton's law, we may use it to determine approximately the amount of these volatile oils which at a certain temperature can exist as vapour in a given volume of air, or rather in a given space. This may be estimated theoretically from the boiling-point of the oil and from the density of its vapour. It may be thus found that eight thousand parts of air at the temperature of  $50^{\circ}$  F. would be capable of holding as vapour one part of the oil of wintergreen, two parts of the oil of meadow-weed, or sixteen parts of the oil of turpentine; but the same air could hold double the amount of these vapours at  $75^{\circ}$  F., and four times as much at  $100^{\circ}$  F. With regard to the ferment oils, the slow rate at which they pass over with the steam during their distillation is a sufficient indication that it requires but a very small portion of their vapours to saturate the air, even during our warmest summers.

From further inquiries as to the origin of these organic bodies, their presence in low marshes will appear still more dependent on heat; and more evidence is thus obtained of their identity with malaria. It is in the warmest climates affording the proper conditions for vegetation that essential oils are produced in the greatest abundance and ferment oils are most rapidly developed by decaying vegetable matter. Accordingly in these regions the air over marshes will most frequently receive almost the full amount of organic vapours which its high temperature enables it to sustain. But the restless condition of the air often prevents it from remaining over these localities long enough to be largely impregnated or poisoned by their exhalations; and the trade-winds contribute much to avert the insalubrity which heat and moisture are ever ready to produce in tropical climates.

In marshes surrounded by high grounds the air is considerably impeded in its movements, and is thus caused to imbibe a larger quantity of their volatile vapours. To the calmness from which such evils arise, the presence of water in a valley contributes in an indirect manner. From the absorption of heat in the formation of aqueous vapour, the lowest air in such localities would have a specific gravity higher than its position would call for, and it would be accordingly less sensitive to those forces which occasion gentle winds. Numerous trees also tend much to the quiescent condition of the air in valleys and marshes; and it is chiefly on this account that the destruction of forests

has contributed so much to check the career of intermittent fever in the New World. It is partly in consequence of the storms with which they are accompanied that excessive rains arrest for a time the effects of malaria; but the mitigation of the evil also depends on the excessive dilution of the volatile oils with water, and their diminished liability to escape into the air.

In dry valleys, in marshes which have been drained, and in lakes having an outlet, the excessive accumulation of the vapour of malaria is in a great measure prevented; but the evils which it is capable of inflicting are generally transferred to other places. Though volatile oils oxidize and become inert in the air, they are slow to change their characters in water; and, accordingly, a river which drains lands teeming with verdure and bearing a luxuriant vegetation must be impregnated with the volatile organic matter which they supply, and which is ready to escape into the air when favoured by the influence of heat and evaporation. It is on this account that the rivers of Italy contribute much to the insalubrity of the lands through which they pass; and many marshes along their banks exhibit the effects of malaria imported from distant localities, and surrendered to the air by the water which is imprisoned and evaporated in stagnant pools. Rivers also find the conditions for diffusing their volatile organic matter when they pass through lakes, or when they spread their waters over the deltas which are so generally formed at their mouths in tideless seas.

As volatile oils slowly escape from the water or change their characters by oxidation, there must be a limit to the distance to which their poisonous characters can be transmitted by rivers. An instance of the effects which these causes produce may be found in the case of the Nile, which in the latter part of its course runs over a thousand miles without receiving a single tributary. Whatever volatile poisons may have contaminated its waters in Equatorial Africa, must have been either expelled by heat or rendered inert by oxidation during its long journey to the Mediterranean. Accordingly its floods do not prevent Egypt from enjoying a comparatively healthy climate. This boon may be partly ascribed to the dryness of the air and the unfrequency of rains in Egypt; but Bussorah, in a region equally dry and free from rains, is rendered extremely pestilential by the inundations of the Euphrates; and the streams which descend along the verdant vales on the southern side of the Elburz mountains, give to the city of Teheran a degree of insalubrity which cannot be ascribed to the luxuriance or to the decay of vegetation in its immediate vicinity.

It is probable that the various volatile oils under consideration



differ much in their effects on human health; but this question can only be settled by observation and experiment. These valuable means of inquiry, however, have been rendered unproductive, as they have been guided by the opinion that all malarious emanations are produced by the plants growing in the moist localities where their evils are manifested. Yet we must consider that, to impregnate the air which changes so frequently over them, marshes must have a very abundant source of these exhalations; and it seems impossible that so large an amount of poisonous matter could exist in their plants without being discoverable by the resources of chemical analysis. But the difficulty may be removed by supposing that the poison consists of volatile vegetable oils collected from wide areas to the low seats of moisture, or occasionally transported away by rivers, and thus enabled to make inroads on the human health in distant regions.

Cincinnati, December 30, 1871.

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XXVI. *Researches on the Electromotive Force in the Contact of Metals, and on the Modification of that Force by Heat.* By E. EDLUND.

[Continued from p. 98.]

§ 4.

*Iron-Copper.*

EXPERIMENT 1. Intensity of the current =  $\tan 23^{\circ} 38'$ .  
The current was reversed after three quarters of an hour.

Deviations.

49.1	}	48.43	Mean 47.22
49.0			
46.6	}	45.20	
42.6			
49.0	}	48.03	
46.7			
49.7			

The numbers under the heading "Deviations" designate the distances in millimetres passed through by the index during 45 minutes. Thus, at the first reversal of the current, the index traversed 49.1 millims. in one direction. The current having been afterwards restored to its previous direction, the index retrograded 49 millims. After the time fixed, the current was again reversed, and the index gave 46.6 divisions of the scale towards the same side as at first. Now, taking the mean of the first and third deviations, and adding to it the second, the sum divided by 2 gives 48.43. All the other means were calculated

by the same process. This artifice was necessary on account of the proper movement of the index independently of the direction of the current. As was said above, in these numbers unity corresponds to about  $0.002^{\circ}$  Celsius.

Exp. 2. Intensity =  $\tan 16^{\circ} 38'$ .

Deviations.

36.9	}	32.10	}	33.48	Mean 32.79
30.5					
30.5					
39.7	}				
24.0					

Exp. 3. Intensity =  $\tan 16^{\circ} 55'$ .

Deviations.

32.4	}	33.42
30.8		
39.7		

Exp. 4. Intensity =  $\tan 5^{\circ} 53'$ .

Deviations.

14.9	}	13.68	}	13.20	Mean 13.44
10.2					
19.4					
7.7	}				
18.0					

Exp. 5. Intensity =  $\tan 5^{\circ} 35'$ .

Deviations.

7.2	}	13.33
16.2		
13.7		

Reducing the deviation in experiment 2 to the same intensity of current as in experiment 3, and, in the same manner, the deviation in experiment 5 to the intensity of the current in exp. 4, we obtain respectively 33.38 and 14.05. Taking the means of these numbers and those directly observed for the same intensities of the current, for  $\tan 16^{\circ} 55'$  the deviation 33.40 is obtained, and for  $\tan 5^{\circ} 53'$  the deviation 13.74.

By calculating these observations in the manner above indicated, we obtain, as mean value,  $\beta = 3.1825$ , and,

from the observation with the current-

				intensity = $\tan 16^{\circ} 55'$ ,	$\alpha = 125.0$
"	"	"	"	= $\tan 5^{\circ} 33'$ ,	$\alpha = 135.6$
"	"	"	"	= $\tan 23^{\circ} 38'$ ,	$\alpha = 136.9$
					Mean = 132.5

Several months after the preceding experiments, and after other metallic combinations had been investigated, the iron-copper combination was again put into the apparatus and submitted to fresh researches with the view of ascertaining, among other things, whether during that long time the apparatus had undergone any alteration. The following results were obtained:—

Exp. 6. Intensity =  $\tan 27^\circ 10'$ .

Deviations.

4.1	}	54.35
100.9		
11.5	}	59.60
98.3		
30.3	}	57.95 Mean 57.30
80.0		
41.5		

Exp. 7. Intensity =  $\tan 10^\circ 8'$ .

Deviations.

11.9	}	23.85	Mean 23.65
38.0			
7.5	}	22.65	
24.3			
34.5			

Submitting these two experiments to the calculation above indicated, we obtain  $\beta = 1.625$ , and  $\alpha = 133.4$ .

Now, taking the mean of the four values of  $\alpha$  obtained, the final result is  $\alpha = 132.73$ . Thus at a current-intensity = 1 =  $\tan 45^\circ$ , there is developed or absorbed, at the surface of contact between copper and iron, a quantity of heat represented, in the units chosen, by the number 132.73.

In the following three experiments the current was reversed at the end of 15 minutes, consequently before the temperature had had time to become stationary. In order to abridge, for all the subsequent experiments we will only give the reduced numbers.

Exp. 8. Intensity =  $\tan 28^\circ$ .

Deviations.

36.35
36.00
35.00
35.93
Mean . . 35.82

Exp. 9. Intensity =  $\tan 10^\circ$ .

Deviations.

15.43
15.20
14.63
15.38
Mean . . 15.16

Exp. 10. Intensity =  $\tan 18^\circ$ .

Deviations.

23.78
24.18
24.45
23.90
Mean . . 24.08

Calculating from these numbers  $\alpha_i$  and  $\beta_i$ , corresponding to  $\alpha$  and  $\beta$  of the other experiments, we obtain, for the mean value,  $\beta_i = 3.1644$ , and,

from the observations with  $\tan 28^\circ$ ,  $\alpha_i = 92.74$ " " "  $18^\circ$ ,  $\alpha_i = 85.60$ " " "  $10^\circ$ ,  $\alpha_i = 90.11$ 

Mean . . 89.48



The passage of the positive current from the copper to the iron produced a cooling at the surface of union.

*Copper-Platinum.*

Exp. 11. Intensity =  $\tan 12^\circ$ . | Exp. 12. Intensity =  $\tan 36^\circ 45'$ .

Deviations.	Deviations.
9.13	31.90
9.73	31.10
10.95	
Mean . . 9.94	Mean . . 31.50

Exp. 13. Intensity =  $\tan 23^\circ 55'$ .

Deviations.
19.38
19.53
Mean . . 19.45

Hence we obtain  $\beta = 0.5445$ , and from

Exp. 11. . . . .	$\alpha = 47.34$
12. . . . .	$\alpha = 48.16$
13. . . . .	$\alpha = 46.15$
Mean . . . . .	47.22

The electromotive force of the copper-platinum combination is therefore expressed by 47.22.

The two following experiments were made at 15-minute intervals.

Exp. 14. Intensity =  $\tan 37^\circ$ . | Exp. 15. Intensity =  $\tan 15^\circ 30'$ .

Deviations.	Deviations.
21.00	7.98
18.43	8.25
20.90	7.80
20.88	9.90
22.15	7.32
Mean . . 20.67	Mean . . 8.25

From these two experiments we obtain  $\beta_1 = 0.3690$ , and  $\alpha_1 = 30.17$ . The passage of the positive current from the platinum to the copper produced a cooling at the surface of union.

*Copper-Aluminium.*

Exp. 16. Intensity =  $\tan 29^\circ 52'$ . | Exp. 17. Intensity =  $\tan 14^\circ 45'$ .

Deviations.	Deviations.
18.10	7.23
14.73	8.05
16.20	9.10
Mean . . 16.34	Mean . . 8.13

Exp. 18. Intensity =  $\tan 41^\circ 50'$ .

Deviations.
23·70
24·70
23·73

Mean . . . 24·04

From these three experiments we obtain  $\beta = 0\cdot4851$ , and from

Exp. 16. . . . .	$\alpha = 31\cdot40$
17. . . . .	$\alpha = 30\cdot65$
18. . . . .	$\alpha = 31\cdot65$
Mean . . . . .	31·23

Some months after the above experiments, the copper-aluminium combination was again introduced into the apparatus, and I made the two following:—

Exp. 19. Intensity =  $\tan 38^\circ 15'$ . Exp. 20. Intensity =  $\tan 20^\circ 55'$ .

Deviations.
21·03
21·30

Mean . . . 21·17

Deviations.
11·82
10·13

Mean . . . 10·98

If we calculate the last two experiments in the usual manner, we obtain  $\beta = 0\cdot3182$ , and  $\alpha = 29\cdot29$ .

Taking the mean of the four values of  $\alpha$  for the copper-aluminium combination, we obtain the final result

$$\alpha = 30\cdot77.$$

The passage of the positive current from the aluminium to the copper produced a cooling at the surface of union.

Three values of  $\alpha$  have been obtained for each of the above-mentioned combinations. The concordance of these values within the limits of possible errors of observation proves that the formulæ of calculation made use of answer their purpose. Another series of observations on the iron-copper combination, for which I used two copper cylinders not silvered (and which is not inserted here, because it is not comparable with the others), furnished five values of  $\alpha$ , likewise agreeing with each other within the limits of errors of observation. The same result was given by the experiments in which the observations took place at intervals of fifteen minutes.

#### Copper-Gold.

Exp. 21. Intensity =  $\tan 36^\circ 40'$ . Exp. 22. Intensity =  $\tan 40^\circ 45'$ .

Deviations.
10·90
10·88

Mean . . . 10·89

Deviations.
13·00
11·98

Mean . . . 12·49

Exp. 23. Intensity =  $\tan 25^\circ 45'$ .

	Deviations.
	5.70
	8.12
Mean . . .	<u>6.91</u>

The calculation of these three experiments gives  $\beta=0$  and  $\alpha=14.5$ . The circumstance that  $\beta$  is here  $=0$ , and consequently the deviations are proportional to the intensity of the current, proceeds doubtless from the very slight elevation of the temperature within the cylinders, this resulting from the minimum development of heat in the two wires, which are good conductors. The cooling of the cylinders becomes then proportional to the excess of temperature of their sides, and this excess, in its turn, proportional to the excess of temperature of the enclosed air.

If we calculate the three experiments supposing the proportionality of the deviations to the intensity of the current, we obtain the following comparison between the observed and the calculated numbers:—

Observed.	Calculated.
6.91	6.99
10.89	10.80
12.49	12.49

Another series of observations, made with the unsilvered cylinders before mentioned, gave, for the copper-gold combination,  $\alpha=12.56$ . The series effected with the same cylinders upon the iron-copper combination gave  $\alpha=115.73$ . Both these values are inferior to those before indicated, viz. 14.5 and 132.73. Dividing these by the preceding, we obtain

$$\frac{14.50}{12.56} = 1.154 \text{ and } \frac{132.73}{115.73} = 1.147,$$

quotients which may be regarded as equal. These four series, therefore, confirm one another.

The following experiments, on the same copper-gold combination, were made with 15-minute intervals between the observations.

Exp. 24. Intensity =  $\tan 33^\circ$ . | Exp. 25. Intensity =  $\tan 43^\circ 15'$ .

Deviations.	Deviations.
6.33	9.20
7.90	10.33
6.98	9.38
7.58	9.45
6.70	9.70
Mean . . .	<u>7.10</u>
	Mean . . . <u>9.61</u>



The calculation of these two experiments gives  $\beta_1=0$ , and that of the observations with intensity of current

$$\begin{aligned} &= \tan 33^\circ \quad . \quad . \quad . \quad \alpha_1 = 10.93 \\ &,, \quad 43^\circ 15' \quad . \quad . \quad \alpha_1 = 10.22 \end{aligned}$$

The point of soldering underwent a cooling when the positive current passed from gold to copper.

*Cadmium-Copper.*

Exp. 26. Intensity =  $\tan 39^\circ 50'$ .    Exp. 27. Intensity =  $\tan 39^\circ 50'$ .

Deviations.	Deviations.
6.00	4.10
5.05	4.38
6.15	4.15
Mean . . 5.73	Mean . . 4.21

Exp. 28. Intensity =  $\tan 24^\circ$ .

Deviations.
2.48
1.70
2.23
1.60
2.10
2.40
1.95
Mean . . 2.07

In experiments 27 and 28 the interval between the observations was 15 minutes. The calculation of  $\beta_1$  and  $\alpha_1$  from these two experiments gives  $\beta=0$ , and, from

$$\begin{aligned} \text{Exp. 27.} \quad . \quad . \quad . \quad \alpha_1 &= 5.05 \\ ,, \quad 28. \quad . \quad . \quad . \quad \alpha_1 &= 4.65 \\ \text{Mean} \quad . \quad . \quad &4.85 \end{aligned}$$

From experiment 26, in which  $\beta$  should be  $=0$ , we obtain  
 $\alpha=6.87$ .

The passage of the positive current from copper to cadmium produced a cooling at the point of contact.

*Copper-Lead.*

Exp. 29. Intensity =  $\tan 36^\circ$ .    Exp. 30. Intensity =  $\tan 20^\circ$ .

Deviations.	Deviations.
14.88	7.53
14.45	7.57
Mean . . 14.67	Mean . . 7.55

Calculating these experiments in the usual way, we obtain  $\beta=0.1419$  and

$$\alpha=20.93.$$

The two following experiments were made with 15-minute intervals between the observations.

Exp. 31. Intensity =  $\tan 36^\circ$ .      Exp. 32. Intensity =  $\tan 20^\circ$ .

Deviations.	Deviations.
10.03	6.80
9.48	4.78
10.80	5.90
10.40	5.38
9.58	
Mean . . 10.06	Mean . . 5.72

Hence we obtain  $\beta=0.8067$  and  $\alpha_1=16.53$ .

The passage of the positive current from lead to copper occasioned a cooling.

#### *Copper-Bismuth.*

Exp. 33. Intensity =  $\tan 8^\circ 20'$ .      Exp. 34. Intensity =  $\tan 12^\circ$ .

Deviations.	Deviations.
115.63	157.65
111.50	174.95
116.78	179.98
Mean . . 114.64	Mean . . 170.86

Exp. 35. Intensity =  $\tan 5^\circ 15'$ .

Deviations.
75.65
71.78
62.90
Mean . . 70.11

Calculating these experiments in the usual way, we obtain  $\beta=0$  and  $\alpha=783.1$ .

If we calculate with this value of  $\alpha$  the deviations for the different intensities of the current, and compare the results with the observed deviations, we obtain:—

Observed.	Calculated.
114.64	114.71
170.86	166.45
70.11	71.96

That  $\beta$  was here  $=0$ , and consequently the deviations proportional to the intensities of the current, results doubtless from the intensities being relatively small. Hence the heat developed in the wires is a minimum, and the excess of temperature in the

cylinders insignificant, which causes the cooling to be proportional to the excess of temperature.

The passage of the positive current from bismuth to copper produced a cooling at the point of union.

*Copper-Tin.*

The observations of the two following experiments were made at intervals of 15 minutes.

Exp. 36. Intensity = $\tan 40^\circ$ .		Exp. 37. Intensity = $\tan 20^\circ$ .	
Deviations.		Deviations.	
15.18		9.60	
14.78		5.40	
12.35		8.98	
16.73		4.18	
17.25		3.25	
16.15		5.55	
11.08		7.35	
Mean . .	14.79	Mean . .	6.33

A second series of observations with the same intensity of the current as in exp. 36, and likewise consisting of seven determinations, gave for the mean 14.40. These two series thus furnish the mean 14.60.

Calculating the results of the above two experiments, we obtain  $\beta_1=0$ , and from

Exp. 36. . . . .	$\alpha_1=17.40$
„ 37. . . . .	$\alpha_1=17.39$
Mean . . . . .	17.40

The cause of the variations when the observations take place at 15-minute intervals has been given above.

The passage of the positive current from tin to copper occasioned a cooling at the point of union.

*Copper-Silver.*

Exp. 38. Intensity of the current =  $\tan 39^\circ 23'$ .

Deviations.	
0.81	
0.69	
0.57	
0.81	
0.86	
Mean . . . .	0.75

For an intensity of current =  $\tan 45^\circ$  we obtain  $\alpha_1=0.91$ .

The passage of the positive current from silver to copper produced a cooling at the point of contact.



*Zinc-Silver.*

The observations were made at intervals of 15 minutes.

Exp. 39. Intensity of the current =  $\tan 39^\circ$ .

Deviations.

0.85

0.93

1.00

Mean . . . 0.93

For an intensity =  $\tan 45^\circ$  we obtain  $\alpha_1 = 1.15$ .

The passage of the positive current from silver to zinc produced a cooling at the point of contact. Thus, from the last two experiments,  $\alpha_1$  should be = 0.24 for the combination of zinc and copper.

*Palladium-Platinum.*

I soldered these two metals together in the supposition that the electromotive force between them would be less than that between palladium and any one of the metals previously investigated. As, however, the length of palladium wire at my disposal was only enough for one of the copper cylinders, I put into the other a platinum wire only. I considered that the inconveniences resulting from slight variations in the intensity of the current could be more easily avoided if the current could traverse the two cylinders in wires offering nearly the same resistance, than if it only passed through one of the cylinders. From what precedes, it is evident that, when there only exists a point of soldering in one of the cylinders, the deviation can only attain to half what it is when in each cylinder there is a point of soldering, of which one becomes heated while the other is cooled. The result must therefore be multiplied by 2 to make it comparable with the preceding ones.

Exp. 40. Intensity of the current =  $\tan 18^\circ 15'$ .

Deviations.

7.53

9.35

Mean . . . 8.44

The intensity not being particularly great, the deviation may be regarded as proportional to it, whence we obtain, for intensity =  $\tan 45^\circ$ ,  $\frac{\alpha}{2} = 25.60$ , and consequently  $\alpha = 51.20$ .

The passage of the positive current from palladium to platinum induced a cooling at the point of soldering.

If, with respect to the metallic combinations for which  $\alpha$  and  $\alpha_1$  have been determined, we compare one of these two quantities with the other, we find that, on the average,  $\alpha = 1.42\alpha_1$ . We

can therefore deduce the former from the latter, and thus obtain the value of  $\alpha$  for those combinations in respect of which it has not been directly determined. In this way we find, for the combinations:—

Iron-copper . . . .	$1.42\alpha_1 = 127.06$
Cadmium-copper . . . .	$= 6.89$
Copper-gold . . . .	$= 15.02$
Copper-lead . . . .	$= 23.47$
Copper-platinum . . . .	$= 42.84$
Copper-tin . . . .	$= 24.71$
Copper-silver . . . .	$= 1.29$
Zinc-copper . . . .	$= 0.34$

The theoretic proof of the signification of Peltier's phenomena demonstrates the production of a cooling at the point of contact when the current circulates in the same direction as the current produced by the electromotive force of that point. As the cooling takes place at the point of contact (between iron and copper, for example) when the current passes from the copper to the iron, this signifies that the electromotive force of the point tends to produce a current from the copper to the iron. In contact, then, the iron becomes electropositive, and the copper electronegative. It is, moreover, well established by experiment that, when several metals, A, B, C, &c., are soldered together so as to form a ring, no current results if all the points of contact have the same temperature. The electromotive force between A and C must therefore be of the same amount as the sum of the forces between A and B and B and C. By the application of these two propositions we obtain, from the preceding determinations, the following electromotive series, commencing with the most positive, and ending with the most negative of the metals investigated. The numbers give the electromotive force of each metal in contact with copper.

	$\alpha$ .	$1.42\alpha_1$ .	Mean.
Iron . . . .	$\left\{ \begin{array}{l} 132.50 \\ 133.40 \end{array} \right\}$	127.06	130.99
Cadmium . . . .	6.87	6.89	6.88
Zinc . . . .	—	0.34	0.34
Copper . . . .	0.00	0.00	0.00
Silver . . . .	—	1.29	1.29
Gold . . . .	14.50	15.02	14.76
Lead . . . .	20.93	23.47	22.20
Tin . . . .	—	24.71	24.71
Aluminium . . . .	30.77	—	30.77
Platinum . . . .	47.22	42.84	45.03
Palladium . . . .	96.23	—	96.23
Bismuth . . . .	783.1	—	783.10

[To be continued.]

XXVII. *Notices respecting New Books.*

*Researches on the Calculus of Variations, principally on the Theory of Discontinuous Solutions ; an Essay to which the Adams Prize was awarded in the University of Cambridge in 1871. By I. TODHUNTER, M.A., F.R.S., late Fellow and Principal Mathematical Lecturer of St. John's College, Cambridge. London and Cambridge : Macmillan and Co. 1871. (Pp. 278.)*

THE terms in which the subject of the essay was originally prescribed are these :—"A determination of the circumstances under which discontinuity of any kind presents itself in the solution of a problem of maximum or minimum in the calculus of variations, and applications to particular instances. It is expected that the discussion of the instances should be exemplified as far as possible geometrically, and that attention be especially directed to cases of real or supposed failure of the calculus." As far as we can venture to sum up the result of Mr. Todhunter's researches in a few words, it is this :—that the discontinuity arises from conditions imposed, either explicitly or implicitly, which limit the generality of the question. Thus, suppose it is required to determine the curve which, taken between fixed limits (A and B), shall give  $\int \phi(q)dx$  a maximum or minimum. There is no difficulty in showing that the required curve is a parabola, and for some forms of  $\phi$  it will be a maximum, for others a minimum ; and there is no discontinuity. But now suppose that we introduce the condition that the curve shall pass through a third point (D). If D is in the parabola already determined, there is again no discontinuity. But if D is not in this parabola, the required curve will consist of two parabolic arcs, one passing from A to D and the other from D to B ; or it may be one passing from A to B, and the other from B to D. This is a case of discontinuity introduced by a condition consciously imposed ; it may, of course, be introduced by a condition unconsciously imposed.

The fundamental principle, which is extensively applied throughout the essay, is this :—"Let there be an integral  $\int \phi dx$  which is required to be a maximum or a minimum, where  $\phi$  is a known function of  $y$  and its differential coefficients with respect to  $x$ . Change  $y$  into  $y + \delta y$  ; then in the usual way we obtain for the variation of the integral to the first order an expression of the form

$$L + \int M \delta y dx,$$

where L depends on the values of the variables and the differential coefficients at the limits of the integration. Now if  $\delta y$  may have either sign, we must have  $M=0$  as an indispensable condition for a maximum or a minimum ; and moreover we must have  $L=0$ . These statements are universally admitted to be true.

"Suppose, however, that owing to some condition in the problem we cannot always give  $\delta y$  either sign ; for example, suppose that



throughout the whole range of the integration  $\delta y$  is *essentially positive*, then it is no longer necessary that  $M$  should vanish. If  $M$  is positive through the whole range of the integration, we are sure of a minimum; and if  $M$  is negative through the whole range of the integration, we are sure of a maximum. We assume, of course, that we are able to satisfy the condition  $L=0$ , or to ensure that  $L$  shall be positive in the former case and negative in the latter case.

“Next suppose that  $\delta y$  may have either sign through part of the range of the integration, but that it is essentially positive during the remainder of the range. Then, if  $M$  vanishes through the former part and is positive through the latter part of the range, we are sure of a minimum; and if  $M$  vanishes through the former part and is negative through the latter part of the range, we are sure of a maximum.”—P. 13.

After exemplifying the discontinuity arising from conditions in several simple cases, Mr. Todhunter goes on to discuss—mainly by the aid of the above principle—various cases of the questions of chief historical importance on the subject, viz. minimum surface of revolution, maximum solid of revolution, brachistochrone under the action of gravity, problem of least action, solid of minimum resistance, area between a curve and its evolute. To each of these questions a chapter is devoted. The way in which they are discussed may be judged of from the contents of a single chapter, viz. that on the brachistochrone (c. 7). The well-known general solution is first mentioned, viz. that, when the body falls from a fixed point (A) to another fixed point (B), the curve is a cycloid with a cusp at A and a horizontal base. The following particular cases are then considered in order:—(a) when the path must pass through a third point C either fixed or on a given curve; (b) when an obstacle with an aperture of given size is interposed between A and B; (c) when C is on a given surface; (d) when the condition is that the moving point must not descend below a horizontal line through B; (e) when it must not pass outside a circular arc of which B is the lowest point, and AB does not exceed a quadrant; (f) when it must not pass inside the circular arc; (g) when the condition is that the radius of curvature shall never be less than a given constant, and that there is no abrupt change of direction. With regard to the discussion of the cases which we have marked (d), (e), (f), (g), Mr. Todhunter says (p. 146) that they completely illustrate the general principles laid down. In each case the discontinuity arises from a condition or conditions imposed. This is, in fact, the theme which he has illustrated in the present essay with the utmost fulness and in a manner worthy of his high reputation as a mathematician. To students of the Calculus of Variations (we fear they are but few) the work cannot fail to prove highly instructive, as it comprises a thorough investigation of a class of cases which, when they occur, have been commonly passed over with the remark that “the process of the Calculus of Variations fails in this case.” It was, in fact, in reference to

a failure of this kind that Mr. Todhunter first enunciated, and in our pages\*, the cardinal principle above quoted.

*Technical Arithmetic and Mensuration.* By C. W. MERRIFIELD, F.R.S. London: Longmans and Co. 1872. (Pp. 308.)

This book is intended for the use of persons who have already learned arithmetic, but who wish to renew and extend their acquaintance with the subject. It consists of a treatise on Arithmetic of 183 pages, a treatise on Mensuration of 64 pages, and an Appendix of 60 pages, comprising Examination Papers, somewhat elaborate Tables of Money, Lengths, Areas, &c., and Answers to Questions. The treatise on Arithmetic gives clearly and concisely an account of the fundamental operations of Arithmetic; it notices with sufficient, but not more than sufficient, fulness the points which have to be dwelt on in oral instruction; and it illustrates the rules by a very considerable number of examples, most of which Mr. Merrifield says he has himself made. Very many of these examples are mere illustrations of rules, such as must of necessity occur in every book of Arithmetic; *e.g.*, "If  $3\frac{3}{7}$  of a share cost £5, what will  $5\frac{5}{6}$  cost?" Many, however, occur here and there which illustrate elementary scientific principles; *e.g.*, "Express miles per hour in metres per second, having given a metre = 39·37 inches." The chief novelty, however, is the last chapter, which treats of "the applications of Arithmetic to Machines, Work, and Motion." This is a very useful chapter; it contains sufficient explanations to render intelligible the examples with which it concludes. The number of these examples might easily have been increased with great advantage. The objection to introducing, except with very great judgment, examples on points of science into a book of Arithmetic, is that they presuppose knowledge which the student probably does not possess. This objection does not, of course, apply in the above case; we are inclined to think it does apply to such examples as (19), p. 53; (16), (17), (25), (26), p. 76—not to say that (25) is inexactly stated, and (26) clearly in the wrong place.

On pp. 66, 67 there is a statement with regard to proportion which is worth notice. In the text occur these words:—"Proportion may include the comparison of ratios which are not definite, and which are incapable of being expressed as fractions." And to this statement the following note is added:—"I do not think it necessary to restrict the idea of proportion to numerical ratio. In fact, it appears to me to be a consequence of

that  
3 shillings : 5 shillings : : 6 yards : 10 yards,

3 shillings : 6 yards : : 5 shillings : 10 yards ;

\* Phil. Mag. June 1866, vol. xxxi. p. 425. The question to which Mr. Todhunter applied the principle was this:—To determine a solid of revolution of given surface so that it may cut the axis of revolution at given points and have a maximum volume. A chapter of the essay is devoted to examining this question and some cases arising out of it.

and that while I do not know what exactly is the meaning of the ratio

3 shillings : 6 yards,

I am enabled to assert that there is a connexion between 5 shillings and 6 yards in the nature of a ratio, and subject to the rules of proportion, which is the same as the connexion between 5 shillings and 10 yards, whatever that may be. To deny this appears to me to imply an objection to concrete arithmetic generally. I do not assert that the ratio is definite." It is much to be regretted that Mr. Merrifield did not omit this altogether, or else that he did not explain his views more fully. Had he taken the latter course, we are sure that his explanation would have been clear of objection. But as the note stands, it is very liable to mislead. We need hardly say that such a proportion as

3 shillings : 6 yards :: 5 shillings : 10 yards

is commonly held to be merely an improper mode of stating that the ratio of 3 shillings to 5 shillings is equal to the ratio of 6 yards to 10 yards; and it is by no means obvious that the common view does not go to the bottom of the matter. At all events, Mr. Merrifield could hardly complain if from his premises we were to draw the conclusion that

3 shillings + 6 yards : 6 yards :: 5 shillings + 10 yards : 10 yards.

But what meaning can be attached to this proportion, beyond a relation between the bare numbers, it is hard to see. Of course if it is merely a question of numbers, there is no difficulty; but this, to all appearance, is not Mr. Merrifield's meaning.

Our notice of the remaining part of the work must be brief. The treatise on Mensuration comprises all the most useful rules for finding areas and volumes; and though there is no attempt to give formal proofs of the rules, their dependence on known geometrical theorems is shown with great clearness, and their use illustrated by a sufficiency of examples. Mr. Merrifield's powers of exposition are perhaps seen to most advantage in this part of the volume. The book as a whole, however, is exceedingly well done; in fact, we are inclined to think that, of all the works hitherto published in Messrs. Longman's series, this comes most nearly up to the notion of a text-book adapted "for the self-instruction of working men."

## XXVIII. *Proceedings of Learned Societies.*

### ROYAL SOCIETY.

[Continued from p. 154.]

Nov. 16, 1871.—General Sir Edward Sabine, K.C.B., President, in the Chair.

THE following communication was read:—

"Considerations on the Abrupt Change at Boiling or Condensing in reference to the Continuity of the Fluid State of Matter." By Professor James Thomson, LL.D.

When we find a substance capable of existing in two fluid states



different in density and other properties while the temperature and pressure are the same in both, and when we find also that an introduction or abstraction of heat without change of temperature or of pressure will effect the change from the one state to the other, and also find that the change either way is perfectly *reversible*, we speak of the one state as being an ordinary gaseous, and the other as being an ordinary liquid state of the same matter; and the ordinary transition from the one to the other we should designate by the terms boiling or condensing, or occasionally by other terms nearly equivalent, such as evaporation, gasification, liquefaction from the gaseous state, &c. Cases of gasification from liquids or of condensation from gases, when any chemical alteration accompanies the abrupt change of density, are not among the subjects proposed to be brought under consideration in the present paper. In such cases I presume there would be no perfect reversibility in the process; and if so, this would of itself be a criterion sufficing to separate them from the proper cases of boiling or condensing at present intended to be considered. If, now, the fluid substance in the rarer of the two states (that is, in what is commonly called the gaseous state) be still further rarefied, by increase of temperature or diminution of pressure, or be changed considerably in other ways by alterations of temperature and pressure jointly, without its receiving any abrupt collapse in volume, it will still, in ordinary language and ordinary mode of thought, be regarded as being in a gaseous state. Remarks of quite a corresponding kind may be made in describing various conditions of the fluid (as to temperature, pressure, and volume) which would in ordinary language be regarded as belonging to the liquid state.

Dr. Andrews (Phil. Trans. 1869, p. 575) has shown that the ordinary gaseous and ordinary liquid states are only widely separated forms of the same condition of matter, and may be made to pass into one another by a course of continuous physical changes presenting nowhere any interruption or breach of continuity. If we denote geometrically all possible points of pressure and temperature jointly, by points spread continuously in a plane surface, each point in the plane being referred to two axes of rectangular coordinates, so that one of its ordinates shall represent the temperature and the other the pressure denoted by that point, and if we mark all the successive boiling- or condensing-points of temperature and pressure as a continuous line on this plane, this line, which may be called the *boiling-line*, will be a separating boundary between the regions of the plane corresponding to the ordinary liquid state and those corresponding to the ordinary gaseous state. But, by consideration of Dr. Andrews's experimental results, we may see that this separating boundary comes to an end at a point of pressure and temperature which, in conformity with his language, may be called the *critical point* of pressure and temperature jointly; and we may see that, from any ordinary liquid state to any ordinary gaseous state, the transition may be effected gradually by an infinite variety of courses passing round outside the extreme end of the boiling-line.

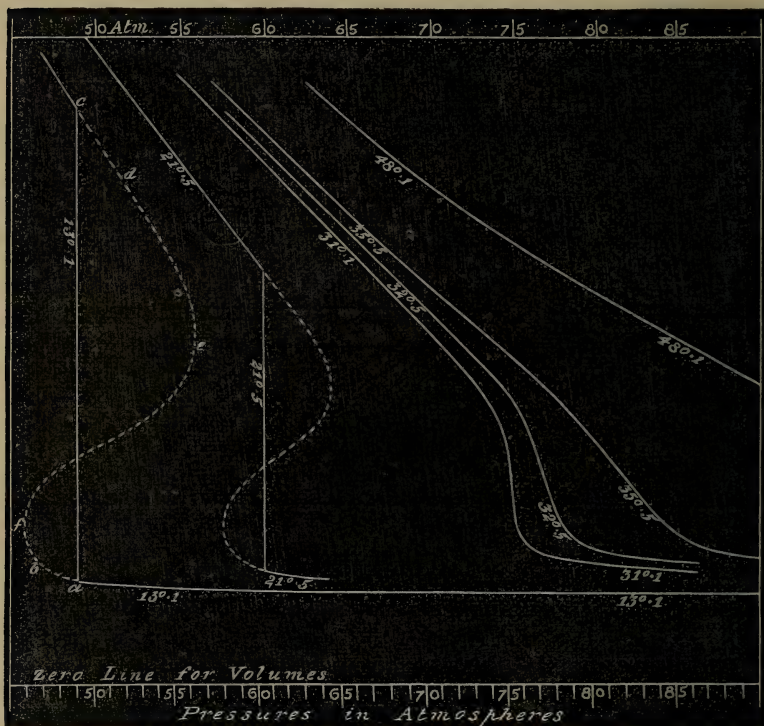
Now it will be my chief object in the present paper to state and support a view which has occurred to me, according to which it appears probable that, although there be a practical breach of continuity in crossing the line of boiling-points from liquid to gas or from gas to liquid, there may exist, in the nature of things, a theoretical continuity across this breach having some real and true significance. This theoretical continuity, from the ordinary liquid state to the ordinary gaseous state, must be supposed to be such as to have its various courses passing through conditions of pressure, temperature, and volume in unstable equilibrium for any fluid matter theoretically conceived as homogeneously distributed while passing through the intermediate conditions. Such courses of transition, passing through unstable conditions, must be regarded as being impossible to be brought about throughout entire masses of fluids dealt with in any physical operations. Whether in an extremely thin lamina of gradual transition from a liquid to its own gas, in which it is to be noticed the substance would not be homogeneously distributed, conditions may exist in a stable state having some kind of correspondence with the unstable conditions here theoretically conceived, will be a question suggested at the close of this paper in connexion with some allied considerations.

It is first to be observed that the ordinary liquid state does not necessarily cease abruptly at the line of boiling-points, as it is well known that liquids may, with due precautions, be heated considerably beyond the boiling temperature for the pressure to which they are exposed. This condition is commonly manifested in the boiling of water in a glass vessel by a lamp placed below, when the temperature of the internal parts of the water, or, in other words, of the parts not exposed to contact with gaseous matter, rises considerably above the boiling-point for the pressure, and the water boils with bumping\*. At this stage it becomes desirable to refer to Dr. Andrews's diagram of curves showing his principal results for carbonic acid, and to consider carefully some of the remarkable features presented by those curves. In doing so, we have first, in the case of the two curves for  $13^{\circ}\cdot 1$  and  $21^{\circ}\cdot 5$ , which pass through the boiling interruption of continuity, to guard against being led, by the gradually bending transition from the curve representing obviously the liquid state into the line seen rapidly ascending towards the curve representing obviously the gaseous state, to suppose that this curved transition is in any way indicative of a gradual transition from the liquid towards the gaseous state. Dr. Andrews has clearly pointed out, in describing those experimental curves, that the slight bend

\* It has even been found by Dufour (Bibliothèque Universelle, Archives, year 1861, vol. xii., "Recherches sur l'ébullition des Liquides") that globules of water floating immersed in oil, so as neither to be in contact with any solid nor with any gaseous body, may, under atmospheric pressure, be raised to various temperatures far above the ordinary boiling-point, and occasionally to so high a temperature as  $178^{\circ}$  C., without boiling.

On this subject reference may also be made to the important researches of Donny, "Sur la cohésion des Liquides et sur leur adhérence aux Corps solides," Ann. de Chimie, year 1846, 3rd ser. vol. xvi. p. 167.—July 28, 1871.

at about the commencement of the rapid ascent from the liquid state is to be ascribed to a trace of air unavoidably present in the carbonic acid—and that if the carbonic acid had been absolutely pure, the ascent from the liquid to the gaseous state would doubtless have been quite abrupt, and would have shown itself in his diagram by a vertical straight line, when we regard the coordinate axes for pressures and volumes as being horizontal and vertical respectively. Now in the diagram here submitted the continuous curves (that is to say, those which are not dotted) are obtained from Dr. Andrews's



diagram, with the slight alteration of substituting, in accordance with the explanations just given, an abrupt meeting instead of the curved transition between the curve for the liquid state and the upright line which shows the boiling stage. Looking to either of the given curves which pass through boiling, and, for instance, selecting the curve for  $13^{\circ}\cdot 1$ , we perceive, from what has been said as to the conditions to which boiling by bumping is due, that for the temperature pertaining to this curve the liquid state does not necessarily end at the boiling pressure for this temperature, and that thus in the diagram the curve showing volumes for the liquid state must not cease at the foot of the upright line which marks the boiling stage of pressure, but must extend continuously, for



some distance at least, into lower pressures in some such way as is shown by the dotted continuation from *a* to *b*. But now the question arises, Does this curve necessarily end at any particular point *b*? We know that the extent of this curve in the direction from *a* towards or past *b*, along which the liquid volume will continue to be represented before the explosive or bumping change to gas occurs, is very variable under different circumstances, being much affected by the presence of other fluids, even in small quantities, as impurities in the fluid experimented on, and by the nature of the surface of the containing vessel, &c.

The consideration of the subject may be facilitated, and aid towards the attainment of clear views of the mutual relations of temperature, pressure, and volume in a given mass of a fluid may be gained, by actually making, or by conceiving to be made, for carbonic acid, from the data supplied in Dr. Andrews's experimental results, a solid model consisting of a curved surface referred to three axes of rectangular coordinates, and formed so that the three coordinates of each point in the curved surface shall represent, for any given mass of carbonic acid, a temperature, a pressure, and a volume which can coexist in that mass. It is to be noticed here that in his diagram of curves the results for each of the several temperatures experimented on are combined in the form of a plane curved line referred to two axes of rectangular coordinates—one of each pair of ordinates representing a pressure, and the other representing the volume corresponding to that pressure at the temperature to which the curve belongs. Now to form a model such as I am here recommending, and have myself made, Dr. Andrews's curved lines are to be placed with their planes parallel to one another, and separated by intervals proportional to the differences of the temperatures to which the curves severally belong, and with the origins of coordinates of the curves situated in a straight line perpendicular to their planes, and with the axes of coordinates of all of them parallel in pairs to one another, and then the curved surface is to be formed so as to pass through those curved lines smoothly or evenly\*. The curved surface so obtained exhibits in a very obvious way the remarkable phenomena of the voluminal conditions at and near the critical point of temperature and pressure, in comparison with the voluminal conditions throughout other parts of the range of gradually varying temperatures and pressures to which it extends, and even throughout a far wider range into which it can in imagination be conceived to be extended. It helps to afford a clear view of the nature and meaning of the continuity of the liquid and gaseous states of matter. It does so by its own obvious continuity throughout its expanse round the end of the range of points of pressure and temperature where an abrupt change of volume can occur by boiling or condensing. On the curved surface

\* For the practical execution of this, it is well to commence with a rectangular block of wood, and then carefully to pare it down, applying, from time to time, the various curves as templets to it, and proceeding according to the general methods followed in a shipbuilder's modelling-room in cutting out small models of ships according to curves laid down on paper as cross sections of the required model at various places in its length.

in the model Dr. Andrews's curves for the temperatures  $13^{\circ}\cdot 1$ ,  $21^{\circ}\cdot 5$ ,  $31^{\circ}\cdot 1$ ,  $32^{\circ}\cdot 5$ ,  $35^{\circ}\cdot 5$ , and  $48^{\circ}\cdot 1$  Centigrade, which afford the data for its construction, may with advantage be all shown drawn in their proper places. The model admits of easily exhibiting in due relation to one another a second set of curves, in which each would be for a constant pressure, and in each of which the coordinates would represent temperatures and corresponding volumes. It may be used in various ways for affording quantitative relations interpolated among those more immediately given by the experiments.

We may now, aided by the conception of this model, return to the consideration of continuity or discontinuity in the curves in crossing the boiling stage. Let us suppose an indefinite number of curves, each for one constant temperature, to be drawn on the model, the several temperatures differing in succession by very small intervals, and the curves consequently being sections of the curved surface by numerous planes closely spaced parallel to one another and to the plane containing the pair of coordinate axes for pressure and volume. Now we can see that, as we pass from curve to curve in approaching towards the critical point from the higher temperatures, the tangent to the curve at the steepest point or point of inflection is rotating, so that its inclination to the plane of the coordinate axes for pressure and temperature, which we may regard as horizontal, increases till, at the critical point, it becomes a right angle. Then it appears very natural to suppose that, in proceeding onwards past the critical point to curves successively for lower and lower temperatures, the tangent at the point of inflection would continue its rotation, and the angle of its inclination, which before was acute, would now become obtuse. It seems much more natural to make such a supposition as this than to suppose that in passing the critical point from higher into lower temperatures the curved line, or the curved surface to which it belongs, should break itself asunder, and should come to have a part of its conceivable continuous course absolutely deficient. It thus seems natural to suppose that in some sense there is continuity in each of the successive curves by courses such as those drawn in the accompanying diagram as dotted curves uniting continuously the curves for the ordinary gaseous state with those for the ordinary liquid state.

The physical conditions corresponding to the extension of the curve from *a* to some point *b* we have seen are perfectly attainable in practice. Some extension of the gaseous curve into points of temperature and pressure below what I have called the boiling- or condensing-line (as, for instance, some extension such as from *c* to *d* in the figure) I think we need not despair of practically realizing in physical operations. As a likely mode in which to bring steam continuing gaseous to points of pressure and temperature at which it would collapse to liquid water if it had any particle of liquid water present along with it, or if other circumstances were present capable of affording some apparently *requisite conditions for enabling it to make a beginning of the change of state*\*, I would suggest the

\* The principle that "the particles of a substance when existing all in one state

admitting speedily of dry steam nearly at its condensing temperature for its pressure (or, to use a common expression, *nearly saturated*) into a vessel with a piston or plunger, all kept hotter than the steam, and then allowing the steam to expand till by its expansion it would be cooled below its condensing-point for its pressure; and yet I should suppose that if this were done with very careful precautions the steam might not condense, on account of the cooled steam being surrounded entirely with a thin film of superheated steam close to the superheated containing vessel. The fact of its not condensing might perhaps best be ascertained by observations on its volume and pressure. Such an experiment as that sketched out here would not be easily made; and unless it were conducted with very great precautions, there could be no reasonable expectation of success in its attempt; and perhaps it might not be possible so completely to avoid the presence of dust or other dense particles in the steam as to make it prove successful. I mention it, however, as appearing to be founded on correct principles, and as tending to suggest desirable courses for experimental researches. The overhanging part of the curve from *e* to *f* seems to represent a state in which there would be some kind of unstable equilibrium; and so, although the curve there appears to have some important theoretical significance, yet the states represented by its various points would be unattainable throughout any ordinary mass of the fluid. It seems to represent conditions of co-existent temperature, pressure, and volume in which, if all parts of a mass of fluid were placed, it would be in equilibrium, but out of which it would be led to rush, partly into the rarer state of gas, and partly into the denser state of liquid, by the slightest inequality of temperature or of density in any part relatively to other parts. I might proceed to state, in support of these views, several considerations founded on the ordinary statical theory of capillary or superficial phenomena of liquids, which is dependent on the supposition of an attraction acting very intensely for very small distances, and causing intense pressure in liquids over and above the pressure applied by the containing vessel and measurable by any pressure-gauge. That statical theory has fitted remarkably well to many observed phenomena, and has sometimes even led to the forecasting of new results in advance of experiment. Hence, although dynamic or kinetic theories of the constitution and pressure of fluids now seem likely to supersede any statical theory, yet phenomena may still be discussed according to the principles of the statical theory; and there may be considerable likelihood that conditions explained

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only, and in continuous contact with one another, or in contact only under special circumstances with other substances, experience a *difficulty of making a beginning of their change of state*, whether from liquid to solid, or from liquid to gaseous, or probably also from solid to liquid," was proposed by me, and, so far as I am aware, was first announced in a paper by me in the Proceedings of the Royal Society for November 24, 1859 (vol. x. p. 158), and in a paper submitted to the British Association in the same year.

In the present paper, at the place to which this note is annexed, I adduce the like further supposition that a *difficulty of making a beginning of change of state* from gaseous to liquid may also probably exist.



or rendered probable under the statical theory would have some corresponding explanation or confirmation under any true theory by which the statical might come to be superseded. With a view to brevity, however, and to the avoidance of putting forward speculations perhaps partly rash, though, I think, not devoid of real significance, I shall not at present enter on details of these considerations, but shall leave them with merely the slight suggestion now offered, and with the suggestion mentioned in an earlier part of the present paper, of the question whether in an extremely thin lamina of gradual transition from a liquid to its own gas, at their visible face of demarcation, conditions may not exist in a stable state having a correspondence with the unstable conditions here theoretically conceived.

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GEOLOGICAL SOCIETY.

[Continued from p. 155.]

Nov. 8, 1871.—Joseph Prestwich, Esq., F.R.S., President,  
in the Chair.

The following communications were read :—

1. A letter from the Embassy at Copenhagen, transmitted by Earl Granville, mentioning that a Swedish scientific expedition, just returned from the coast of Greenland, had brought home a number of masses of meteoric iron found there upon the surface of the ground. These masses varied greatly in size ; the largest was said to weigh 25 tons.

MR. DAVID FORBES having recently returned from Stockholm, where he had the opportunity of examining those remarkable masses of native iron, took the opportunity of stating that they had been first discovered last year by the Swedish arctic expedition, which brought back several blocks of considerable size, which had been found on the coast of Greenland. The expedition of this year, however, has just succeeded in bringing back more than twenty additional specimens, amongst which two were of enormous size. The largest, weighing more than 49,000 Swedish pounds, or about 21 tons English, with a maximum sectional area of about 42 square feet, is now placed in the hall of the Royal Academy of Stockholm ; whilst, as a compliment to Denmark, on whose territory they were found, the second largest, weighing 20,000 lbs., or about 9 tons, has been presented to the museum of Copenhagen.

Several of these specimens have been submitted to chemical analysis, which proved them to contain nearly 5 per cent of nickel, with from 1 to 2 per cent of carbon, and to be quite identical, in chemical composition, with many *aërolites* of known meteoric origin. When polished and etched by acids, the surface of these masses of metallic iron shows the peculiar figures or markings usually considered characteristic of native iron of meteoric origin.

The masses themselves were discovered lying loose on the shore, but immediately resting upon basaltic rocks (probably of Miocene age), in which they appeared to have originally been imbedded ; and

not only have fragments of similar iron been met with in the basalt, but the basalt itself, upon being examined, is found to contain minute particles of metallic iron, identical in chemical composition with that of the large masses themselves, whilst some of the masses of native iron are observed to enclose fragments of the basalt.

As the chemical composition and mineralogical character of these masses of native iron are quite different from those of any iron of terrestrial origin, and altogether identical with those of undoubted meteoric iron, Professor Nordenskjöld regards them as *aërolites*, and accounts for their occurrence in the basalt by supposing that they proceeded from a shower of meteorites which had fallen down and buried themselves in the molten basalt during an eruption in the Miocene period.

Notwithstanding that these masses of metallic iron were found lying on the shore between the ebb and flow of tide, it has been found, upon their removal to Stockholm, that they perish with extraordinary rapidity, breaking up rapidly and falling to a fine powder. Attempts to preserve them by covering them with a coating of varnish have as yet proved unsuccessful; and it is actually proposed to preserve them from destruction by keeping them in a tank of alcohol.

2. "On the Geology of the Diamond-fields of South Africa." By Dr. John Shaw, of Colesberg. Communicated by Dr. Hooker, F.R.S., F.G.S.

The author described the general structure of the region in which diamonds have been found. He considered that the diamonds originally belonged to some metamorphic rock, probably a talcose slate, which occupied the heights during a late period of the "trappean upheaval" to which he ascribed the origin of the chief physical features of the country. This upheaval was followed by a period of lakes, the traces of which still exist in the so-called "pans" of the region; the Vaal river probably connected a chain of these lakes; and it is in the valley of the Vaal and the soil of the dried-up "pans" that the diamonds are found. The author referred also to the frequent disturbance and removal of the diamantiferous gravels by the floods which prevail in these districts after thunder-storms.

3. "On the Diamond-gravels of the Vaal River, South Africa." By G. W. Stow, Esq., of Queenstown, Cape Colony. Communicated by Prof. T. Rupert Jones, F.G.S.

The author described the general geographical features of the country in which diamonds have been found, from Mamusa on the south-west to the headwaters of the Vaal and Orange Rivers. He then indicated the mode of occurrence of the diamonds in the gravels, gravelly clays, and boulder-drifts of the Vaal valley, near Pniel, including Hebron, Diamondia, Cawood's Hope, Gong Gong, Klip Drift, Du Toit's Pan, and other diggings. By means of sections, he showed the successive deepening of the Vaal valley and the gradual accumulation of gravel-banks and terraces, and illustrated the enormous catchment area of the river-system, with indications of the geological structure of the mountains at the headwaters. The specimens sent by Mr. Stow, as interpreted by Prof. T. R. Jones, showed that both igneous and metamorphic rocks had supplied the material

of these gravels. The author concluded that a large proportion of these materials have travelled long distances, probably from the Draakensberg range; but, whether the original matrix of the diamonds is to be found in the distant mountains or at intermediate spots in the valleys, the worn and crushed condition of some of the diamonds indicates long travel, probably with ice-action. Polished rock-surfaces and striated boulders, seen by Mr. Gilfillan, were quoted in corroboration of this view.

Nov. 22, 1871.—The Rev. Thomas Wiltshire, M.A., F.L.S.,  
in the Chair.

The following communications were read:—

1. "Notes on some Fossils from the Devonian rocks of the Witzenberg Flats, Cape Colony." By Prof. T. Rupert Jones, F.G.S.

In this paper the author noticed some Devonian fossils like those of the Bokkeveld, found on Mr. Louw's farm on the Witzenberg Flats, Tulbagh. *Orthoceras vittatum*, Sandberger, was added to the South-African list of fossils. The fossils under notice were stated by the author to help to substantiate the late Dr. Rubidge's view, that the old schists termed "Silurian" by Bain are of Devonian age, and continuous across the Colony. Their presence in the Witzenberg Flats was also shown to be conclusive against the idea of Coal-measures being found there.

2. "On the Geology of Fernando Noronha (S. lat. 3° 50', W. long. 32° 50')." By Alexander Rattray, M.D. (Edin.), Surgeon R.N. Communicated by Prof. Huxley, F.R.S., V.P.G.S.

The author described the general geological structure of Fernando Noronha and the smaller islands which form a group with it. The surface-rock was described as a coarse conglomerate, composed of rounded basaltic boulders and pebbles, in a hard, dark-red, clayey matrix. This overlies a hard, dark, fine-grained basalt, which forms the most striking of the bluffs, cliffs, and outlying rocks. The highest peaks in the group consist of a fine-grained, light-grey granite. The author remarked upon the possible relation of the geology of these islands to that of the neighbouring continent of South America, and stated that there is evidence of the islands having been elevated to some extent at a comparatively recent period.

3. "Note on some Ichthyosaurian remains from Kimmeridge Bay, Dorset." By J. W. Hulke, Esq., F.R.S., F.G.S.

The author noticed some teeth found, with a portion of an Ichthyosaurian skull, in the Kimmeridge Clay of Dorsetshire. The fragments of the snout were said to indicate that it was about three feet long, and proportionally stout. The author indicated the characters by which these teeth were distinguishable from those of various known species of *Ichthyosaurus*, and stated that they approached most closely to those of the Cretaceous *I. campylodon*.

4. "Appendix to a 'Note on a new and undescribed Wealden Vertebra,' read 9th February, 1870, and published in the Quarterly Journal for August in that year." By J. W. Hulke, Esq., F.R.S., F.G.S.

The author generically identified this vertebra with *Ornithopsis*,



Seeley, *Streptospondylus*, Owen, and *Cetiosaurus*, Owen, taking the last to be typified by the large species in the Oxford Museum. He remarked that if this be the type of *Cetiosaurus*, *C. brevis*, Owen, can hardly belong to it, as the trunk-vertebræ are described as being of a totally different structure.

December 6, 1871.—Joseph Prestwich, Esq., President, in the Chair.

The following communications were read:—

1. "On the presence of a raised beach on Portsdown Hill, near Portsmouth, and on the occurrence of a Flint Implement at Downton." By Joseph Prestwich, Esq., F.R.S., President.

The author noticed a section observed by him in a pit ten miles westward of Bourne Common and five miles inland in a lane on the north side of East Cams Wood. It is situated at an elevation of 300 feet above the sea-level, and shows some laminated sands with seams of shingle, overlying coarse flint-shingle with a few whole flints, which the author regarded as a westward continuation of the old sea-beach which has been traced from Brighton, past Chichester, to Bourne Common. A flint flake was found by the author at the bottom of the superficial soil in this pit. The author also noticed the occurrence of a flint implement of the type of those of St. Acheul in a gravel near Downton in Hampshire. This gravel capped a small chalk-pit; and its elevation above the river Avon was about 150 feet. Two gravel-terraces occur between this pit and the river,—one 40–60, the other 80–110 feet above the level of the latter.

2. "On some undescribed Fossils from the 'Menevian Group of Wales.'" By Henry Hicks, Esq., F.G.S.

In this communication the author gave descriptions of all the fossils hitherto undescribed from the Menevian rocks of Wales. The additions made to the fauna of the Lower Cambrian rocks (Longmynd and Menevian groups) by the author's researches in Wales during the last few years now number about fifty species, belonging to twenty-two genera, as follows:—

Trilobites, 10 genera and 30 species.

Bivalved and other Crustaceans, 3 genera and 4 species.

Brachiopods, 4 genera and 6 species.

Pteropods, 3 genera and 6 species.

Sponges, 1 genus and 4 species.

Cystideans, 1 genus and 1 species.

By adding to these the Annelids, which are plentiful also in these rocks, we get seven great groups represented in this fauna, the earliest known at present in this country. By referring to the Tables published in M. Barrande's excellent new work on Trilobites, it will be seen that this country also has produced a greater variety, or, rather, representatives of a greater number of groups from these early rocks than any other country. The species described included *Agnostus*, 5 species; *Arionellus*, 1 species; *Erinnys*, 1 species; *Holcephalina*, 1 species; *Conocoryphe*, 2 species; *Anopolenus*, 2 species; *Cyrtotheca*, 1 species; *Stenotheca*, 1 species; *Theca*, 2 species; *Protocystites*, 1 species, &c. The author also entered into a consideration of the range of the genera and species in these

early rocks, and showed that, with the exception of the *Brachiopods*, *Sponges*, and the smaller *Crustacea*, the range was very limited.

A description of the various beds forming the Cambrian rocks of St. David's was also given, and proofs adduced to show that frequent oscillations of the sea-bottom took place at this early period, and that the barrenness of some portions of the strata, and the richness of other parts, were mainly attributable to these frequent changes.

### XXIX. *Intelligence and Miscellaneous Articles.*

ON SIGNALS OBSERVED IN A WIRE JOINING THE EARTH-PLATES IN THE NEIGHBOURHOOD OF A THIRD EARTH-PLATE USED FOR A TELEGRAPHIC CIRCUIT. BY G. K. WINTER, TELEGRAPH ENGINEER, MADRAS RAILWAY.

**I**N the course of some experiments on earth-currents an effect was observed which has led to the discovery of a fact which may be of some practical value, besides throwing additional light upon the part played by the earth in telegraphic circuits.

Although several telegraph-wires pass through Arconum, only two are terminal at that station,—one connecting it with Cuddapah, a town lying 120 miles to the north-west; and the other, which is very little used, connects it with Conjeveram.

Two earth-plates were imbedded in the ground in a line running east and west and nearly radial to the office earth-plate; they were about a quarter of a mile apart, and the nearest was about the same distance from the office plate. A wire was erected on separate poles, connecting the two experimental earth-plates. When a reflecting galvanometer, similar to those used for cable signalling, was inserted in this circuit, there was a pretty strong deflection; but riding on this deflection, as it were, were distinct signals, which were ultimately, on comparison, found to agree perfectly with the signals passing in the Cuddapah wire.

The experiments were made in a building at least 150 yards from the telegraph office; and the poles carrying the experimental wire were at least that distance from those carrying the Cuddapah wire. It is utterly impossible, therefore, that any accidental leakage could account for the signals. On other wires running in other directions and varying from one to two miles in length, the same effects were observed; so that the currents going to earth at the office earth-plate must radiate from it through the earth, and the signals in the experimental wires must have been derived from these radiations.

The signals I have now made perfectly legible by using a thick wire reflecting galvanometer, filling the mirror-chamber with water to steady the needle and mirror.

I have, in conclusion, to express my thanks to Mr. Lundy, of the British Indian Extension Telegraph Company, for having kindly lent me a reflecting galvanometer and one of his staff accustomed to read by the mirror. I may also mention that, when speaking to him on the subject of the signals I had observed, he said he thought they were due to a wire that was terminal at Arconum. I was myself of the same opinion, but had not at the time actually made the comparison.

Arconum, Jan. 27, 1872.

ON A REMARKABLE FAULT IN THE NEW RED SANDSTONE OF WHISTON, CHESHIRE. BY PROFESSOR EDWARD HULL, F.R.S., DIRECTOR OF THE GEOLOGICAL SURVEY OF IRELAND.

The position of this fault is marked on the Geological Survey maps of Lancashire (1-inch map 80 N.W.) as forming the boundary between the little isolated tract of coal-measure one mile west of Rainhill station and the New Red Sandstone. The fault ranges in a nearly meridional direction; and on the west the upper coal-measures with *Spirorbis limestone*, first discovered by Mr. Binney, F.R.S., are brought to the surface, and on the east the upper mottled sandstone of the Bunter division of the Trias.

The Corporation of St. Helens, in order to increase the water-supply of the borough, commenced sinking a well, on Mr. Hull's recommendation, at a distance of 200 yards from the fault in the New Red Sandstone, close to Cumber Lane bridge\*; this well was carried down 75 yards, and from the bottom a bore-hole, 18 inches diameter, was driven 35 yards further; but at 104 yards from the surface it passed through the fault and entered hard micaceous sandstone of a purple colour belonging to the upper coal-measures.

As the horizontal distance from the outcrop of the fault where it crosses the railway is 200 yards, and the depth 104 yards, it appears that the slope of the fault is about 2 horizontal to 1 vertical, or  $28^{\circ}$  from the horizontal.

The usual slope of the fault in South Lancashire being 2 vertical to 1 horizontal, such a result was unexpected; and as the thickness of New Red Sandstone was thus reduced below the calculated amount, the quantity of water obtained (about 400,000 gallons per day) was consequently much less than that required and anticipated. Other means have been adopted for increasing the supply.—Communicated by the Author, having been read before the Royal Geological Society of Ireland, February 14, 1872.

#### DISPLACEMENT OF THE SPECTRAL LINES BY THE ACTION OF THE TEMPERATURE OF THE PRISM. BY M. BLASERNA.

I have still a fact to communicate which is not unimportant for spectroscopy. On looking at the sun's spectrum through a bisulphide-of-carbon prism, I observed that the Fraunhofer's lines were considerably displaced. This fact is not new in reference to liquids. It was observed in Verdet's laboratory. As regards the refractive index, it has long been known that it changes with the temperature.

But it has been universally assumed in reference to solids that these changes are unimportant. I had therefore the idea of working with the substances most important for spectroscopy, and used a flint-glass prism by Duboscq. The displacements of the lines are here far feeble, yet distinctly visible. This is easily manifested if the prism be heated in sunshine, then rapidly inserted in a spectroscopic in the shade and adjusted to any line. As the prism cools, the line is displaced, and in such a manner, in the case of glass, that *the deflection increases as the temperature sinks*; the reverse being the case with bisulphide. If the temperature of the prism is not uniform, the Fraunhofer lines become confused and but barely visible.

\* This site was selected, not as being the best for water-supply, but the best available.



With my flint-glass prism of  $60^\circ$  I measured the displacements of the double line of sodium; but the measurements were within a small interval of temperature. To give you an idea of our climate, I may mention that for six weeks, during which I chose the most favourable hours of the day and of the night, I could obtain no greater interval than at most  $5\frac{1}{2}^\circ$ . But by using an excellent theodolite-spectrometer of Starke in Vienna, by which measurements to a second could be effected, I could observe that the line  $D'$  was displaced  $3'$  for  $1^\circ$  Centigrade. This displacement is considerable; for the distance between  $D$  and  $D'$  amounts, in my apparatus, to  $12'$ . From this it follows that a change of  $4^\circ$  C. is sufficient to remove  $D$  to the place of  $D'$ . This source of error is the more important because it easily occurs—if, for instance, an observation made in full sunshine be compared with one made in the shade, or one made at midday be compared with one in the night or one in the morning.

There is thus only one good method of spectroscopic comparison, namely the superposition of the spectra. In other cases the spectroscope must be graduated and the temperature frequently determined, which is difficult when, for instance, the sun is observed. For this reason more than one spectroscopic measurement needs repeating.—*Bibliothèque Universelle*, August 1871.

COLOURED GELATINE PLATES AS OBJECTS FOR THE SPECTROSCOPE. BY E. LOMMEL.

In order to avoid the inconvenience of using solutions in glass vessels in demonstrating the phenomena of absorption of soluble colouring-matters, I use gelatine *laminæ* which are coloured with the matters in question. To protect them from wearing out, from air and dust, they are enclosed between two colourless glass plates; and even with delicate colouring-matters they keep perfectly. A compendious collection of the most varied colouring-matters may thus be made, which are always handy for demonstration, whether for the objective projection of absorption-spectra, or for subjective observation by the spectroscope. We can, for instance, at any time exhibit the changes which the spectrum of the colouring-matter of the blood undergoes by various agents without needing to perform the lengthy operations with fresh blood.

The changes of the absorption-spectra for continually thicker layers of the colouring medium may be quickly and conveniently effected by superposing on each other a continually larger number of gelatine plates of the same thickness and the same intensity of colour. The phenomena of a wedge-shaped layer may be imitated by superposing a large number of feebly coloured *laminæ*.

The production of perfectly homogeneous and transparent *laminæ* can be effected even with colouring-matters which are soluble in alcohol but not in water, as, for instance, aniline colours and chlorophyl. Chlorophyl gelatine, however, does not give the spectrum of the alcoholic chlorophyl solution which was used for colouring the aqueous gelatine solution, but that of *solid* chlorophyl as shown by leaves in transmitted light. I have not been able to observe any such difference between the spectrum of the solidified colouring-matter in the gelatine as compared with that of its solution.—Poggendorff's *Annalen*, October 1871. [Thin plates of mica might be even more suitable for this purpose.—Ed. Phil. Mag.]

Fig. 2.

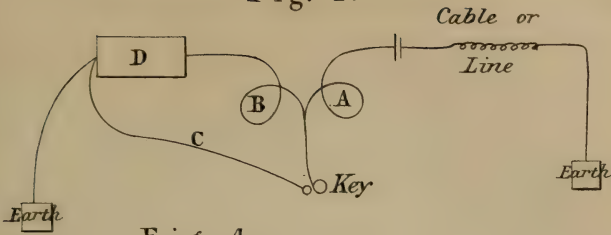


Fig. 4.

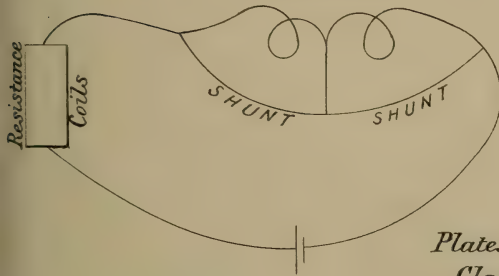
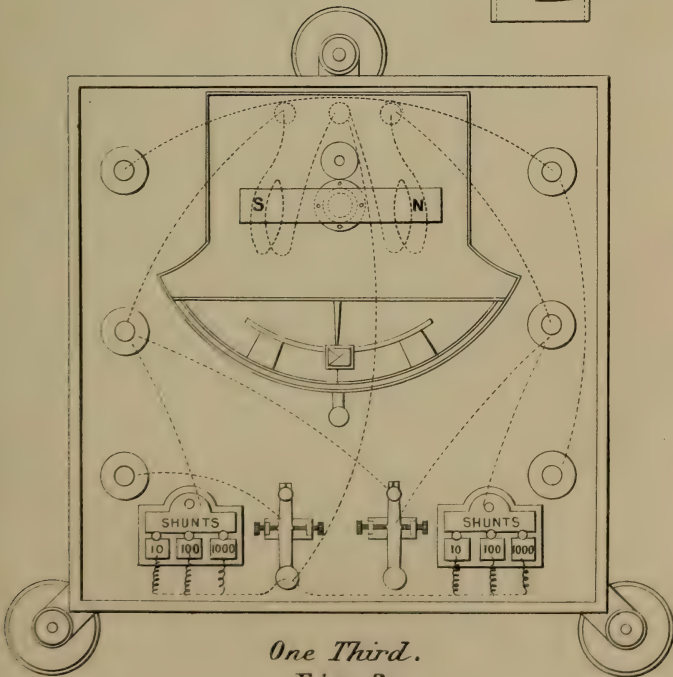
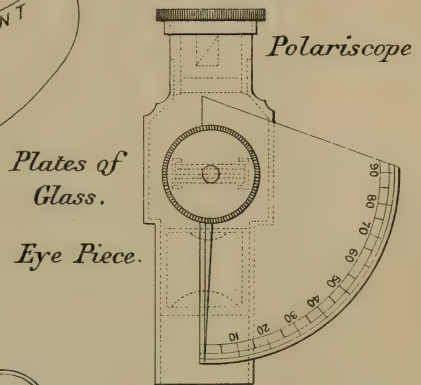


Fig. 1.



One Third.

Fig. 3.





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AND  
JOURNAL OF SCIENCE.

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[FOURTH SERIES.]

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APRIL 1872.

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XXX. *On the Relations between the Atomic Hypothesis and the Condensed Symbolic Expressions of Chemical Facts and Changes known as Dissected (Structural) Formulæ.* By C. R. A. WRIGHT, D.Sc., Lecturer on Chemistry in St. Mary's Hospital Medical School\*.

ON comparing the published views and statements of different chemists on the subject of the Atomic Hypothesis, it is evident that in some instances the same meanings are not always given to the words employed in stating the fundamental positions of the various lines of argument, and that the meaning and scope of the phrase "Atomic Hypothesis" are very differently understood by different thinkers. Thus highly contradictory statements are to be met with—one chemist considering that the "law of multiple proportions has no existence apart from the atomic theory," and that the atomic theory is "the very life of chemistry," while others deny *in toto* the truth of the first proposition, and say that "the science of chemistry does not require or prove the atomic theory."

The object aimed at in the following pages is to show:—first, that the main salient facts and generalizations on which chemical philosophy is founded are capable of expression in words, and of representation by the symbols in ordinary use, without in any way involving the ideas bound up in the hypothesis of the existence of material atoms as devised by Dalton (in its chemical relations) and subsequently extended; and secondly, that this hypothesis, though affording a clear *raison d'être* for many of these facts, is yet incapable of accounting readily for all of them,

\* Communicated by the Author.

*Phil. Mag.* S. 4. Vol. 43. No. 286. April 1872.

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—in other words, that the conceptions involved in this hypothesis are both unnecessary and insufficient.

In pursuance of this object it is clearly necessary, first, to enumerate the fundamental facts in language that clearly expresses them without at the same time involving any theoretical views whatever, and then to enunciate the theory and show how far it accounts for these facts. In most writings on the subject it unfortunately happens that terms such as *atom*, *molecule*, *structure*, &c. are employed in stating fundamental data, these terms necessarily involving the ideas and notions of the very hypothesis under consideration; to avoid the *petitio principii* thus rendered imminent, it is desirable to eliminate such terms altogether from the language employed in stating the fundamental facts, and the way in which they are expressed and alluded to by the ordinary symbols.

1. The volume occupied by 1 gramme of hydrogen under given circumstances as to temperature and pressure in any experiment is termed a "*Relative Vapour-volume Unit*, or more briefly, a "*Volume*."

2. The specific gravities of vapours (vapour-densities) with respect to hydrogen mean *the numbers of grammes that a volume of the gases or vapours weighs*, the pressure and temperature being respectively such that the compounds in question are æriform.

Thus the volume occupied by 1 gramme of hydrogen at 0° and 760 millims. is 11·2 litres; 11·2 litres of oxygen at this pressure and temperature weigh 16 grammes; hence 16 is the specific gravity of oxygen.

The volume occupied by 1 gramme of hydrogen at temperature  $t^{\circ}$  and pressure  $p$  millims. is  $11\cdot2 \times (1 + 0\cdot00365 \times t) \times \frac{760}{p}$ .

This volume of water (the pressure and temperature being such that the water is æriform) weighs 9 grammes; hence the vapour-density of water is 9.

3. A homogeneous body, when submitted to analytical processes, is either capable of furnishing simultaneously more, or not more, than one substance dissimilar in properties to the original body. In the first case the body is said *to be a compound, to be composed of the several substances* which it can thus furnish on analysis; in the latter case it is called an *element*; and if it furnish one substance dissimilar to the original body, the original substance is said to be an *allotropic modification of this element*.

Thus ozone analyzed by heat or by induction-sparks yields but one body dissimilar from ozone; this new product is termed oxygen, and ozone is said to be an allotropic modification of the element oxygen: oxygen itself, never having been analyzed into any two substances, is therefore termed an element.

Mercuric oxide when heated, however, yields not only oxygen identical with that from ozone, but also metallic mercury; hence mercuric oxide is said to be a compound of mercury and oxygen.

The questions as to whether oxygen and mercury are present *as such* in mercuric oxide, whether both are essentially altered in the combined state, whether bodies generally are simply force determined in some or other different ways, &c., are speculations with which the chemist who wishes to deal solely with *facts* has nothing to do. Mercuric oxide is a name given to a body which, on heating, furnishes the two bodies termed oxygen and mercury respectively; and this fact is referred to when it is said that mercuric oxide is a *compound* of mercury and oxygen; this, and the converse proposition that mercury and oxygen can under certain conditions reproduce the original mercuric oxide, are referred to when it is said that mercury and oxygen *unite together* to form mercuric oxide, or that mercury and oxygen *are components* of mercuric oxide.

4. Bodies that are not decomposed by a rise in temperature are either aëriiform at common temperatures, gasefiable at higher temperatures, or non-volatile at measurable temperatures. Many elements which are themselves non-volatile yield gasefiable compounds with elements belonging to the other two classes; the composition by weight of these compounds is known by analysis or synthesis. From this composition and the vapour-densities of the compounds the volumetric ratio between the compounds and the gasefiable components is known. In many instances this ratio admits of being also directly determined by experimenting with the components in the gaseous state.

Thus water is found to consist of 88.89 per cent. oxygen and 11.11 per cent. of hydrogen: the vapour-densities of steam, oxygen, and hydrogen are respectively 9, 16, and 1. Hence

1 volume of water-vapour consists of  $\frac{11.11}{100} \times \frac{9}{1}$  volumes of hy-

drogen and  $\frac{88.89}{100} \times \frac{9}{16}$  volumes of oxygen, *i. e.* of 1 volume of

hydrogen and  $\frac{1}{2}$  volume of oxygen: volumetric analysis or synthesis of steam verifies this calculation.

Experiment shows that a volume of the vapour of almost all gasefiable compounds contains the gasefiable elements in the

proportion of  $\frac{1}{2}, \frac{2}{2}, \frac{3}{2}, \frac{4}{2}, \frac{5}{2}, \dots$  volumes, *i. e.* that *two* volumes

of the compound vapour contain 1, 2, 3, 4, 5, ... volumes of the gasefiable components. *Two volumes is therefore chosen as the relative unit of volume for compound vapours.*



There are exceptions to this rule, viz. :—compounds of arsenic and phosphorus, which contain only multiples of  $\frac{1}{2}$  volume of phosphorus or arsenic vapour in 2 volumes of compound vapour, and not multiples of 1 volume; also compounds that dissociate. The former compounds are excluded from consideration on the assumption that the bodies examined in the vaporous condition under the names of phosphorus and arsenic are not the true elements, but only allotropic modifications of them, bearing to them an analogy similar to that of ozone to oxygen, a view corroborated by the facts known with regard to sulphur (§ 8).

5. *The smallest number of grammes of an element contained in two volumes of the homogeneous vapour of any of its compounds is termed the COMBINING NUMBER of the element.* Thus the smallest number of grammes of carbon contained in the homogeneous vapour of any of its compounds is 12; similarly of oxygen, 16; of hydrogen, 1; and so on. 12, 16, and 1 are therefore called the combining numbers of carbon, oxygen, and hydrogen respectively.

6. Many compounds contain in two volumes of vapour a larger number of grammes of some of the components than the combining number; the number of grammes thus contained, however, is *always found to be a multiple of the combining number.* Thus compounds of hydrogen contain 2, 3, 4, ... grammes of hydrogen in two volumes of vapour, but no intermediate fractional numbers of grammes; so of carbon, 24, 36, 48, ... grammes, but no intermediate numbers; so of oxygen, 16, 32, 48, 64, ... grammes, but no intermediate numbers. This is referred to under the name of the "*Law of multiple proportions.*"

7. The following convention is employed to indicate symbolically the composition of compounds. A letter from the name of each element (or more letters than one) is taken as a symbol to represent *not merely that element, but also a relative weight of the element in the proportion of its combining number*; thus

H	means a relative weight of 1 part of hydrogen;
C	"              "              12 parts of carbon;
O	"              "              16 parts of oxygen.

Compounds are indicated by the juxtaposition of the symbols of the component elements. When two volumes of the vapour of the compound contain a multiple of the combining number of grammes of any element, this fact is expressed by applying a *suffix* to the symbol of the element, the value of the suffix being the numerical value of the multiple: in accordance with the law of multiple proportions, this value is always an integer, a multiple of unity.

Thus water is expressed by the compound symbol  $H^2O^1$ ,

because two volumes of steam contain two volumes of hydrogen and one volume of oxygen. Necessarily, therefore, a compound symbol expresses the quantitative composition by weight as well as by volume; the expression  $H^2O^1$  indicates that in water the hydrogen and oxygen are in the proportion of  $2 \times 1$  parts of the first to  $1 \times 16$  parts of the second.

These collocations of symbols and suffixes are termed *Formulae*; usually the suffix 1 is omitted for brevity. In the majority of known formulæ the suffix-values are not high, comparatively few being known where the values exceed 4, except in the case of carbon compounds.

8. Since two volumes of hydrogen weigh 2 grammes, the formula of hydrogen is written  $H^2$ . Similarly two volumes of oxygen weigh  $2 \times 16$  grammes, and therefore the formula of oxygen is  $O^2$ ; so of chlorine,  $Cl^2$ ; of nitrogen,  $N^2$ ; of sulphur at high temperatures,  $S^2$ , ... This is tantamount to an expression of the fact, that in the case of such elements as these the specific gravity with respect to hydrogen is expressed by the combining number.

This rule, however, does not hold good for all gasifiable elements, nor for allotropic modifications. Thus two volumes of the homogeneous vapour of mercury-compounds contain 200 grms. of mercury as the minimum number; 200 is therefore the combining number: two volumes of mercury-vapour, however, only weigh 200 grammes, and hence the formula of mercury is  $Hg$  and not  $Hg^2$ ; *i. e.* the specific gravity of its vapour with respect to hydrogen is only half its combining number. On the other hand, phosphorus and arsenic are found to have the formulæ  $P^4$  and  $As^4$ ; *i. e.* the vapour-densities of these elements are double their combining numbers respectively. So sulphur at low temperatures has the formula  $S^6$  and ozone  $O^3$ . Of the different modifications of an element, that which thus gives the lowest formula is regarded as being the element itself, and those of higher formulæ as modifications of it; and hence it is possible that ordinary arsenic and phosphorus are not the true elements, but only allotropic modifications.

9. In some instances homogeneous compounds yield, on heating, non-homogeneous vapours, which, however, reproduce the original compounds on cooling: the non-homogeneity of the vapour is found by physical or other tests (*e. g.* diffusion in the case of sulphuric acid or ammonium chloride, amalgamation of gold leaf in the case of calomel-vapour). In these cases *Dissociation* is said to take place. Such vapours always lead to formulæ different from those ascribable to the compounds from analogy and other considerations, and frequently indicate the combining number of an element as less than the minimum number derived

from other compounds of the element that do not dissociate. Thus the vapour-density of calomel (dissociated) indicates the formula  $\text{HgCl}$ ; but the generalization of even variation in valency (§ 22) indicates double this, or  $\text{Hg}^2\text{Cl}^2$ ; and the vapour-densities of sulphuric acid, phosphorus pentachloride, and ammonium chloride would represent the combining numbers of sulphur, phosphorus, nitrogen, and chlorine as respectively 16, 15.5, 7, and 17.75—*i. e.* the halves of the minimum numbers occurring in undissociated compounds.

*Compounds that dissociate, or are believed to do so, must therefore be excluded in the determination of the combining number of an element.*

10. In the case of the elements Mercury, Sulphur, Selenium, Tellurium, Bromine, Iodine, Phosphorus, Arsenic, Antimony, Bismuth, Tin, Osmium, Zinc, and probably others, the combining numbers deduced from the gasefiable compounds of these elements are found to be *approximately inversely proportional to the specific heats of these elements in the solid state*, or in other words, specific heat  $\times$  combining number = constant = 6.6 nearly.

A similar law is found to hold with some of the gaseous elements; only in this case the value of the constant is different, as might be expected from the different physical conditions; thus the permanent gases oxygen, hydrogen, and nitrogen give the constant 3.4, and the condensable gases chlorine and bromine-vapour the constant 4.3.

Applying this principle to those elements which do not yield gasefiable compounds, it is found that when each element is denoted by a symbolic letter (or letters), and each symbol is made to represent a relative weight of the element it indicates approximately equal to  $\frac{6.6}{S}$  (where  $S$  is the specific heat of the solid element), the quantitative composition of all compounds of these elements may be represented by collocations of symbols and suffixes similar in character to the formulæ deduced for the gasefiable compounds from their composition and vapour-density, and specially characterized by the fact that the suffix-values are always integers and rarely high numbers.

The term "formula" is therefore extended to the collocations of symbols and suffixes that thus express the quantitative composition of non-volatile compounds, the term "combining number" being similarly extended to the relative numbers approximately equal to  $\frac{6.6}{S}$ .

In many instances it happens that a relation between the values of the suffixes applied to a given symbol in several differ-



ent formulæ of this kind exists similar to that which exists in the case of the formulæ of volatile compounds, and to which the term "law of multiple proportions" is given; this law is therefore extended so as to include such cases of non-volatile substances.

11. From the specific heats of sodium, magnesium, iron, and platinum the combining numbers of these elements are respectively fixed as 23, 24, 56, and 198. The composition of some of the compounds of these elements with chlorine are then expressible by the formulæ  $\text{NaCl}$ ,  $\text{MgCl}^2$ ,  $\text{FeCl}^2$ ,  $\text{PtCl}^2$ ,  $\text{FeCl}^3$ ,  $\text{PtCl}^4$ , in which instances the law of multiple proportions is noticeable as holding. It is manifest that any of the formulæ  $\text{Na}^2\text{Cl}^2$ ,  $\text{Mg}^2\text{Cl}^4$ ,  $\text{Fe}^2\text{Cl}^4$ ,  $\text{Pt}^2\text{Cl}^4$ ,  $\text{Fe}^2\text{Cl}^6$ ,  $\text{Pt}^2\text{Cl}^8$ , or  $\text{Na}^3\text{Cl}^3$ ,  $\text{Mg}^3\text{Cl}^6$ ,  $\text{Fe}^3\text{Cl}^6$ ,  $\text{Pt}^3\text{Cl}^6$ ,  $\text{Fe}^3\text{Cl}^9$ ,  $\text{Pt}^3\text{Cl}^{12}$ , &c. would equally well express the composition by weight of these compounds. Vapour-density being the sole means of deciding on the formula of a body (apart from reasons based on analogy), and being inapplicable in such cases, the *simplest integral formula* is chosen, not because there is any evidence in its favour, but solely for the sake of simplicity. This amounts, therefore, to making the supposition *that two volumes of vapour of the body, if obtainable, would contain weights of the constituents indicated by the simplest integral formula.*

In some cases, however, the progress of discovery has shown that the formula deduced from this supposition is lower than the correct one: thus the composition of ferric chloride is expressible by the simplest integral formula  $\text{FeCl}^3$ , which was accordingly attributed to it until the vapour-density of the compound was taken, when the formula was found to be really  $\text{Fe}^2\text{Cl}^6$ .

Similarly, arguments from analogy and other considerations sometimes lead to the adoption of a formula higher than that deduced from the above convention. Thus two carbon chlorides whose simplest integral formulæ are  $\text{CCl}^2$  and  $\text{CCl}^3$  exist; vapour-density shows that the true formulæ are  $\text{C}^2\text{Cl}^4$  and  $\text{C}^2\text{Cl}^6$ ; hence it is inferred that, as ferric chloride has the formula  $\text{Fe}^2\text{Cl}^6$ , ferrous chloride is  $\text{Fe}^2\text{Cl}^4$  and not  $\text{FeCl}^2$ . Again the simplest integral formulæ for the two copper chlorides are  $\text{CuCl}$  and  $\text{CuCl}^2$ ; but the generalization that variation in valency proceeds by even differences (§ 22) leads to the higher formula  $\text{Cu}^2\text{Cl}^2$  for the first compound. Again, the simplest integral formula for codeine is  $\text{C}^{18}\text{H}^{21}\text{NO}^3$ ; but the first product obtainable from this substance by the action of hydrogen chloride is  $\text{C}^{36}\text{H}^{43}\text{ClN}^2\text{O}^6$ ; hence it is inferred that the true formula for codeine is double the simplest integral one, *i. e.* is  $\text{C}^{36}\text{H}^{42}\text{N}^2\text{O}^6$ .

12. The formulæ of bodies are thus fixed from consideration of the following points:—Quantitative volumetric composition in the case of volatile compounds (*i. e.* the weights of the compo-

nents contained in two volumes of vapour); specific heat of non-volatile elements; and generalizations, analogies, and conventions of various kinds, occasionally leading to higher formulæ than those deduced from the convention as to the simplest integral formula.

When fixed in these ways, several remarkable relations between the formulæ of bodies and their physical and other properties are found to exist. Thus a number of crystalline bodies of analogous and similar formulæ are found to crystallize in forms of the same geometrical structure, and are frequently capable of forming different portions of the same regular geometric crystal. Such bodies are said to be *isomorphous*; and the law of isomorphism (Mitscherlich's law) may be simply expressed thus—*similar formula, similar crystal*. Thus potassium nitrate and calcium carbonate, whose formulæ are analogous, viz.  $\text{KNO}_3$ ,  $\text{CaCO}_3$ , are not merely isomorphous, but also isodimorphous. The alums, the magnesium- and copper-sulphate families, and many others might also be cited.

13. The following connexion is found to exist between the *specific gravities* of solid and liquid substances and the formulæ attributed to them\*: if certain numbers be assigned to each elementary symbol in a formula, these numbers sometimes varying in different classes of compounds, but being constant for all members of the same class, the sum got by multiplying each of these numbers by the suffix of the symbol to which it refers and adding together the products is always approximately equal to the quotient obtained by dividing by the specific gravity of the body in question (measured under certain given conditions, as at the boiling-point in the cases of liquids) the sum obtained by multiplying the combining number of each symbol in the formula by its suffix and adding together the products thus obtained.

This relation is more simply expressed by stating that the *specific volume of a compound can be calculated when its formula is known together with certain numbers depending only on the nature of the component elements, and in some instances on the class of compound formed*. Thus oxygen has the number 7·8 attached to it in compounds analogous to water, alcohol, &c.; but the number 12·2 in such bodies as aldehyde, acetone, &c. Carbon has similarly the number 11, and hydrogen 5·5. Then the specific volume of water at its boiling-point is

$$2 \times 5 \cdot 5 + 7 \cdot 8 = 18 \cdot 8, \text{ which approximately } = \frac{2 \times 1 + 16}{0 \cdot 9579},$$

where 0·9579 is the specific gravity of water at its boiling-point.

\* Kopp, *Ann. der Chem. und Pharm.* vol. xevi. p. 153.

So the specific volume of alcohol ( $C^2H^6O$ ) is

$$2 \times 11 + 6 \times 5.5 + 7.8 = 62.8, \text{ approximately } = \frac{2 \times 12 + 6 + 16}{0.74},$$

and that of aldehyde ( $C^2H^4O$ ) is

$$2 \times 11 + 4 \times 5.5 + 12.2 = 56.2, \text{ approximately } = \frac{2 \times 12 + 4 + 16}{0.78};$$

where 0.74 and 0.78 are the specific gravities of alcohol and aldehyde at their boiling-points respectively.

Similarly for other elements. Thus in mercaptan and analogous bodies sulphur has the number 22.6, and in other substances 28.6 attached to it.

It is frequently noticeable that the specific volumes of isomorphous compounds (which accordingly have analogous formulæ) are nearly identical; analogy of composition cannot, however, be inferred from identity of specific volume, as many instances are known where bodies of utterly dissimilar formulæ have nearly identical specific volumes.

14. A similar connexion exists between the *specific refractive energies* and the formulæ of compounds\*. If certain numbers be attributed to each elementary symbol in a formula, these numbers sometimes varying with the class of compound, but being constant for all compounds of the same class, the sum obtained by multiplying each of these numbers by the suffix of the symbol to which it refers and adding together the products is always approximately equal to the product obtained by multiplying by the specific refractive energy of the body in question the sum obtained by multiplying the combining number of each symbol in the formula by its suffix and adding together the products.

This relation is more simply expressed by stating that the *refraction-equivalents of compounds are calculable when their formulæ are known, and likewise certain numbers depending only on the nature of the component elements, and in some instances on the class of compound formed.* Thus attributing to carbon, in such compounds as ether, alcohol, &c., the number 5.0, to hydrogen in similar bodies 1.3, and to oxygen 3.0, the refraction-equivalent of ether ( $C^4H^{10}$ ) is  $4 \times 5.0 + 10 \times 1.3 + 3.0 = 36.0$ , approximately  $= (4 \times 12 + 10 + 16) \times 0.49$ , where 0.49 is the specific refractive energy of ether.

In other classes of compounds different numbers must be attributed to one or other of these elements: thus in benzene-derivatives, carbon has a higher number than 5.0; in hydracids, hydrogen has the number 3.5 attached to it.

15. In many instances, especially in organic bodies, the *boiling-point* of a liquid and its formula are connected after a some-

\* Gladstone, C. S. J. [2]vol. viii. p. 101.



what analogous fashion. Thus by comparing formulæ which are analogous, but differ in the value of the suffixes, it is noticed that a difference in the boiling-point occurs precisely proportionate to the alterations in the suffix-values multiplied respectively by certain numbers attributed to carbon, hydrogen, oxygen, &c., the values of these numbers frequently depending on the class of compound examined\*.

A somewhat similar circumstance occurs as regards the melting-points of bodies of analogous formulæ, though the numerical connexions do not appear to be so distinctly marked.

The specific heats of solid substances of analogous formulæ are also frequently found to be inversely proportionate to the sum of the product obtained by multiplying the combining numbers of each symbol in the formula by its suffix and adding together the products.

16. When the elementary symbols are arranged in the order of their combining numbers fixed by the foregoing considerations, they are found to succeed one another in a remarkable sequence as regards their chemical analogies, forming sets of (usually) 8 numbers each so arranged that the 1st, 2nd, 3rd, ... members of each set exhibit considerable analogy†.

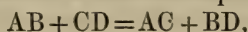
General formula of oxygen compound.							
$R^2 O$ .	$R^2 O^2 = RO$ .	$R^2 O^3$ .	$R^2 O^4 = RO^2$ .	$R^2 O^5$ .	$R^2 O^6 = RO^3$ .	$R^2 O^7$ .	$R^2 O^8 = RO^4$ .
General formula of hydrogen compound.							
		$RH^4$ .		$RH^3$	$RH^2$	$RH$ .	
H=1							
Li=7	Be=9.4	B=11	C=12	N=14	O=16	F=19	
Na=23	Mg=24	Al=27.3	Si=28	P=31	S=32	Cl=35.5	
K=39	Ca=40	.....	Ti=48	V=51	Cr=52	Mn=55	{ Fe = 56 Co = 59 N = 59
Cu=63	Zn=65	.....	.....	As=75	Se=78	Br=80	
Rb=85	Sr=87	Yt=88 ?	Zr=90	Nb=94	Mo=96	.....	
Ag=108	Cd=112	In=113	Sn=118	Sb=122	Te=125 ?	I=127	{ Ru = 104 Rh = 104 Pd = 106
Cs=133	Ba=137	Di=138 ?	Ce=140 ?				
.....	.....	Er=178 ?	La=180 ?	Ta=182	W=184	.....	
Au=199	Hg=200	Tl=204	Pb=207	Bi=208			{ Os = 195 Ir = 197 Pt = 198
.....	.....	.....	Th=231	.....	U=240		

\* Kopp, *Ann. der Chem. und Pharm.* vol. xvi. pp. 2 & 330; vol. xviii. pp. 267 & 367.

† Newland's "Law of Octaves," *Chemical News*, vol. xiii. p. 113. Odling, 'Lectures,' *passim*. Mendelejeff, *Ann. der Chem. und Pharm.* (Supplement) vol. viii. p. 133. Zaengerle, *Deut. Chem. Ges. Ber.* vol. iv. p. 571.

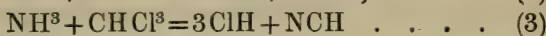
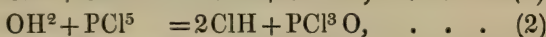
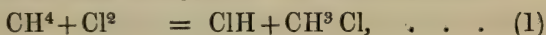
The well-known numerical relationships between the combining numbers of allied elements (*e. g.*  $\text{Li} + \text{K} = 2\text{Na}$ ,  $\text{K} + \text{Cs} = 2\text{Rb}$ ,  $\text{Cl} + \text{I} = 2\text{Br}$ ,  $\text{P} + \text{Sb} = 2\text{As}$ , &c.) and the regular progression in physical and chemical properties observable in members of the same family according as their combining numbers are higher (*e. g.*  $\text{Cl}$ , gas;  $\text{Br}$ , liquid;  $\text{I}$ , solid;  $\text{Mg}$ ,  $\text{Ca}$ ,  $\text{Sr}$ ,  $\text{Ba}$ :  $\text{N}$ ,  $\text{P}$ ,  $\text{V}$ ,  $\text{As}$ ,  $\text{Sb}$ ,  $\text{Bi}$ : &c.) are either consequences of, or closely related to, these "periodic laws."

17. Chemical reactions may be expressed by equations when one or more formulæ occur on each side of the sign of equality; the most general form in which such equations may be put is



where A, B, C, D represent portions of formulæ which are associated together differently in the resulting products from what they are in the generators.

If AB, CD, AC, BD are the formulæ of two generators and two products respectively, the reaction is said to be one of *double decomposition*; and to the portions of formulæ A, B, C, D the terms *Groups* or *Radicals* are applied. Thus the equations



represent double decompositions; and on inspecting them it is noticeable that the formulæ of the resulting products may be deduced from those of the generators by *replacing* certain symbols in one generator by certain others in the other generator, *i. e.* *substituting one group for another*. Thus in equation (1) the symbol Cl is substituted for H in the formula  $\text{CH}^4$ , producing  $\text{CH}^3\text{Cl}$ ; or the group or radical  $\text{CH}^3$  is replaced by Cl, forming ClH; while Cl is replaced in  $\text{Cl}^2$  by H, forming ClH; or the radical  $\text{CH}^3$  is substituted for Cl in  $\text{Cl}^2$ , producing  $\text{CH}^3\text{Cl}$ .

Similarly in equation (2),  $\text{PCl}^3$  replaces  $\text{H}^2$  in  $\text{H}^2\text{O}$ , forming  $\text{PCl}^3\text{O}$ ; and  $\text{Cl}^2$  replaces O in  $\text{H}^2\text{O}$ , forming  $\text{H}^2\text{Cl}^2$  or  $2\text{HCl}$ ; or otherwise, O replaces  $\text{Cl}^2$  in  $\text{PCl}^5$ , forming  $\text{PCl}^3\text{O}$ ; while  $\text{H}^2$  replaces  $\text{PCl}^3$ , forming  $\text{H}^2\text{Cl}^2$  or  $2\text{HCl}$ .

Again in equation (3), N replaces  $\text{Cl}^3$  in  $\text{CHCl}^3$ , producing  $\text{CHN}$ ; or  $\text{H}^3$  replaces CH, forming  $\text{H}^3\text{Cl}^3$  or  $3\text{HCl}$ ; while CH replaces  $\text{H}^3$  in  $\text{NH}^3$ , forming  $\text{NCH}$ ; or  $\text{Cl}^3$  replaces N, producing  $\text{Cl}^3\text{H}^3$  or  $3\text{ClH}$ .

A radical, therefore, is one or more symbols and suffixes transferable from one formula to another, the process indicated by such transfer being a given chemical change or reaction; hence the employment of the word radical necessarily involves the idea of a reaction. The term combining number is applied to a radical to indicate the sum obtained by multiplying the

combining number of each elementary symbol in the radical by its suffix and adding together the products: thus the combining number of  $\text{CH}^3$  is  $12 + 3 \times 1 = 15$ ; of  $\text{PCl}^3$ ,  $31 + 3 \times 35.5 = 137.5$ ; of  $\text{CH}$ ,  $12 + 1 = 13$ , and so on. When it is said that 13 parts of the radical  $\text{CH}$  replace 14 parts of nitrogen, or that 16 of oxygen are replaced by 71 of chlorine, or that 137.5 parts of the radical  $\text{PCl}^3$  replace 2 of hydrogen, or that 35.5 of  $\text{Cl}$  replace 1 of hydrogen, reference is made to the existence of such chemical reactions as are represented symbolically by the equations (1), (2), (3) above.

18. From the comparison of such reactions as those represented above, it is found that 35.5 parts of chlorine always are replaced by, or are substituted for, 1 part of hydrogen; that 16 of oxygen replace 71 of chlorine or 2 of hydrogen, and so on. This is expressed by saying that 35.5 parts of chlorine are equivalent to 1 of hydrogen; that 16 parts of oxygen are equivalent to 71 of chlorine or to 2 of hydrogen, *i. e.* that 8 of oxygen are equivalent to 35.5 of chlorine or to 1 of hydrogen, and so on.

1, 8, and 35.5, ... are then said to be the *equivalents* of hydrogen, oxygen, and chlorine respectively; similarly  $\frac{13}{3}$  is the equivalent of the radical  $\text{CH}$  in the equation (3);  $\frac{137.5}{2}$  of the radical  $\text{PCl}^3$  in equation (2).

In a number of instances the following physical law is found to hold:—*the passage of a given quantity of electricity through an electrolyte causes the evolution of equivalent quantities of the radicals into which it decomposes the electrolyte, no manner what be the nature of the electrolyte.* Thus if a given quantity of electricity evolve 1 part of hydrogen, it would evolve 8 of oxygen, 35.5 of chlorine, and so on, if made to electrolyze compounds of those elements respectively.

19. In some instances the following rule holds,—*that the heat evolved by the action of a given weight of a substance on equivalent quantities of analogous bodies is approximately constant*; thus almost exactly the same amount of heat is produced by the action of a given weight of sulphuric acid on equivalent quantities of each of the hydrates of potassium, sodium, lithium, thallium, barium, calcium, strontium, and magnesium\*.

20. In other instances it is noticeable that the greater the amount of heat given out in any number of a series of analogous reactions involving equivalent quantities of the substances used, the more stable is the resulting product; thus equivalent quantities of potassium, zinc, copper, and mercury evolve the following numbers of calories when they unite with 1 part of

\* Thomsen, *Deut. Chem. Ges. Ber.* vol. iv. p. 308.



chlorine :—

Potassium . . .	=2943
Zinc . . . . .	=1427
Copper . . . . .	= 859
Mercury . . . . .	= 822

Accordingly metallic copper precipitates mercury from its chloride, forming copper chloride; zinc decomposes this, precipitating the copper just dissolved and forming zinc chloride, which in its turn is capable of decomposition by potassium.

In cases where equivalent quantities of different substances produce approximately the same amounts of heat by union with a given weight of some other body, it is often noticeable that none of the first substances will decompose a compound of one of them and the other body; thus 35.5 grammes of chlorine

by combination with 32 of zinc evolve 50296 calories

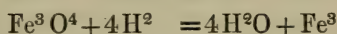
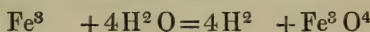
„ „ 28 „ iron „ 49651 „

or approximately the same. Accordingly it is found that metallic zinc has no action on ferrous chloride, and that metallic iron has no action on zinc chloride.

Similarly  $O^4$  in uniting with  $Fe^3$  evolves 265776 calories

while  $H^8$  „ „  $O^3$  „ 271048 „

Experiment shows that by passing steam over red-hot iron, or hydrogen over red-hot magnetic iron oxide, the reciprocal reactions

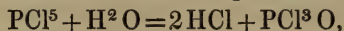


take place according to circumstances. The effect which the different physical properties of bodies have in inducing reactions is well exemplified in the decomposition of potassium sulphate by barium hydrate; the production of the insoluble barium sulphate gives rise to the evolution of a further quantity of heat, *i. e.* the latent heat of barium sulphate; and hence the reaction ensues, although no action might be anticipated, as barium hydrate and potassium hydrate evolve equal numbers of calories with a given quantity of sulphuric acid, so far as the heat of the reaction alone is concerned.

21. A radical being the name given to an assemblage of symbols (one or more) that occurs in the formula of each of two substances derivable from one another by double decomposition, the term *Valency* is applied to the radical, to indicate the quotient obtained by dividing the combining number of the radical by its equivalent in the particular reaction in question, *i. e.* to indicate the number of grammes of hydrogen, or  $\frac{1}{35.5}$  of the

number of grammes of chlorine which the combining number of grammes of the radical can directly or indirectly replace.

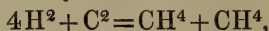
Thus O in  $\text{PCl}^3\text{O}$  is *Bivalent*, its equivalent in the reaction



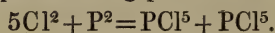
being 8; and  $\frac{16}{8} = 2$ .

CH is *Trivalent* in NCH because it replaces  $\text{H}^3$  in  $\text{NH}^3$ .

Similarly C is said to be *Quadrivalent* in  $\text{CH}^4$ , this compound being viewed as formed by the reaction



where C replaces  $\text{H}^4$  in  $4\text{H}^2$ ; and P is said to be *Quinquevalent*, in  $\text{PCl}^5$ , this compound being producible by the reaction



22. The same symbol or aggregation of symbols is sometimes found to exhibit different valencies in different compounds: in most instances, however, the *alteration in valency occurs by even differences*; i. e. a radical is always either an *Artiad* or a *Perissad* (Odling).

In the formulæ of non-volatile bodies, apparent exceptions can always be made to conform to the rule by employing a higher formula; in the case of the few volatile bodies which apparently do not conform to this rule, dissociation may in some instances be supposed to occur, so that the irregular formula is perhaps incorrect.

Valency =	0	...	.....	.....	Hg	Pt	...	...	$\text{Fe}^2$				
	1	...	.....	.....	IH	...	...	...	...	N(OAg)			
	2	$\text{OH}^2$	$\text{SH}^2$	.....	$\text{HgI}^2$	$\text{PtCl}^2$	CO	$\text{C}^2\text{H}^2$	...	NO			
	3	...	.....	.....	$\text{ICl}^3$	...	...	...	...	$\text{NH}^3$			
	4	$\text{OAg}^4$	$\text{SI}(\text{C}^2\text{H}^5)^3$	.....	...	$\text{PtCl}^4$	$\text{CH}^4$	$\text{C}^2\text{H}^4$	$\text{Fe}^2\text{Cl}^4$	$\text{NO}^2$			
	5	...	.....	$\text{IO}^2(\text{OH})^2$	...	...	...	...	...	$\text{NH}^4\text{Cl}$	$\text{VCl}^4$		
	6	...	.....	.....	...	...	...	$\text{C}^2\text{H}^6$	$\text{Fe}^2\text{Cl}^6$	.....	$\text{VOCl}^3$	$\text{WCl}^5$	
	7	...	.....	.....	...	...	...	...	...	.....	.....	$\text{WC}^6$	
	8	...	.....	.....	...	$\text{PtK}^2\text{Cl}^6$	...	...	...	$\text{N}(\text{CH}^3)^4\text{Cl}^2\text{I}$	.....	.....	

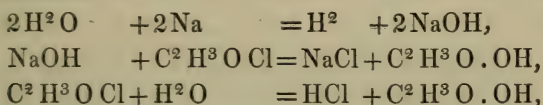
In the above examples it is noticeable that the valency of the symbols O, S, I, Hg, Pt, C,  $\text{C}^2$ ,  $\text{Fe}^2$  always alters by an even number. The symbols N, V, W are exceptions to this rule; in the case of nitrogen, however, there is experimental proof that the vapour which yields the formula  $\text{NO}^2$  either has dissociated, or has undergone some change analogous to dissociation, inasmuch as the vapour when examined under different conditions of pressure and temperature is found to indicate the formula  $\text{N}^2\text{O}^4$ \*. It might be supposed that nitric oxide has

\* Playfair and Wanklyn, Chem. Soc. Journ. [1] vol. xv. p. 156.

similarly the true formula  $N^2 O^2$ , the gas as usually met with being in this quasi-dissociated condition. The existence of the chlorides and oxychlorides &c. of vanadium and wolfram, recently investigated by Roscoe\*, however, shows that dissociation cannot be considered to take place in all the apparent exceptions to this very general rule.

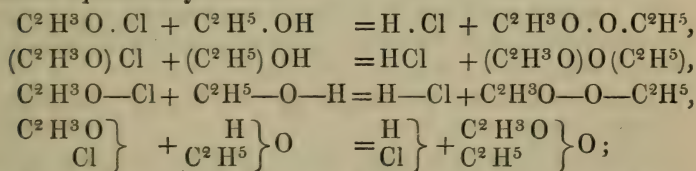
23. Observation shows that the following rule applies to all formulæ with extremely few exceptions:—If the suffix of each symbol in the formula be multiplied by the valency which the symbol exhibits in the majority of its compounds (*e. g.* 2 for oxygen, 4 for carbon), the sum of the products thus obtained is an even number; from which it follows that the sum of the suffixes of the perissad symbols in a formula is always an even number (Gerhardt's 'Law of Even Numbers').

24. Before it can be asserted that two bodies contain a common radical, a reaction must be found whereby one of them is convertible into the other; thus water, caustic soda, and acetic acid are all said to contain the radical hydroxyl (OH), because of the reactions

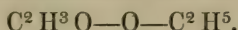


and not because the formulæ  $H^2O$ ,  $NaOH$ ,  $C^2H^4O^2$  all contain the symbols O and H. Conversely  $C^2H^4O$  (aldehyde) is said not to contain hydroxyl (although the symbols O and H are present in the formula), because no reaction of this kind is known.

Reactions such as the above are alluded to and expressed by dividing the formula of each generating substance into two portions or radicals, which are separated from one another by dots, lines, parentheses, &c. Thus the action of acetyl chloride on alcohol, producing hydrogen chloride and acetic ether, is represented equationally thus:—



and these reactions (and others of similar character) are referred to when it is said that acetic ether has the *Dissected Formula*

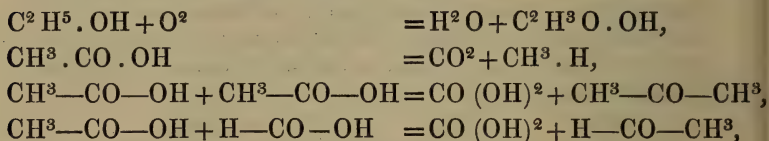


When several reactions of the same substance are compared,

\* Phil. Trans. 1868 and 1869; Chem. News, vol. xxv. p. 61.



it is frequently noticed that the formula is dissected in different ways in accordance with different reactions. Usually all these ways may be expressed in one single formula by carrying the dissection so far as to break up the formula into several groups, of which fewer or more are taken together to form the particular radical required in any given reaction. Thus the action of acetyl chloride on water gives rise to the dissected formula  $C^2H^3O.OH$  for acetic acid; the production of this acid from methyl cyanide gives rise to the differently dissected formula  $CH^3.CO^2H$ ; both of these reactions, however, can be expressed by carrying the dissection to this point  $CH^3.CO.OH$ . It is usually found that formulæ thus dissected into very simple groups are capable of expressing all the reactions of the body in question; thus the formula  $CH^3.CO.OH$  expresses not only the above two reactions, but also the following,



&c.; *i. e.* all reactions lead (usually) to the same final dissection.

25. In a few instances, however, it is found that different reactions lead to different final dissections. Thus the reactions of ethylene dibromide and of glycol lead to the dissected formula for the latter  $OH-CH^2-CH^2-OH$ ; the reactions of aldehyde lead to the formula  $CH^3-CO-H$ . When glycol is dehydrated by zinc chloride, aldehyde is produced; and this would lead to the formula for glycol,  $CH^3-CH(OH)^2$ .

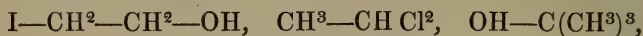
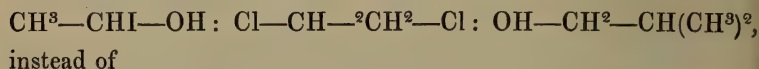
Similarly the reactions,

Glycol iodhydrin and zinc methyl = zinc iodomethide and isopropyl alcohol,

Ethylidene dichloride and potassium cyanide, product with caustic potash gives ordinary succinic acid,

Trimethyl carbinol and oxygen give water and isobutyric acid,

indicate the dissected formulæ of the first-named generators to be respectively



these latter formulæ being those derived from the majority of the reactions of these bodies.

Cases of this kind are, comparatively speaking, so rare as not.

sensibly to detract from the extreme usefulness of dissected formulæ as deduced from and briefly representing a large number of generalizations, laws, and reactions, as "*memoria technica*" recalling the essential chemical differences exhibited between isomeric substances and the analogies between bodies of the same classes, and, in fine, as representing in extremely small compass, and calling to mind, all the salient points of chemical philosophy generally.

26. The relations of the atomic hypothesis to the facts previously mentioned as summed up in, and referred to by, the symbolic expressions termed dissected formulæ may be briefly stated thus:—

The atomic hypothesis supposes that the forces known to us at present are incapable of carrying the mechanical or chemical divisions of elementary matter beyond a certain limit—that in the case of all elementary substances small portions, indivisible by these forces, exist, these portions being accordingly termed *atoms*: by the union together of these primary atoms compound groups of atoms termed *molecules* are formed, which molecules, as a rule, do not consist of any large number of component atoms. The relative weights of the ultimate elementary atoms are denoted by the combining numbers of the elements, hence termed *atomic weights*; thus the atom of carbon is 12 times as heavy as that of hydrogen, that of oxygen 16 times as heavy, and so on.

Homogeneous compounds consist of a large number of precisely similar molecules, and hence must always exhibit the same composition.

The assumption known as *Avogadro's law* is made, that equal bulks of all homogeneous gases and vapours contain, when measured under the same circumstances as to temperature and pressure, the same number of molecules. From this it follows that the formulæ of bodies, as previously defined, express the relative numbers of the different kinds of atoms present in the molecules. It does not necessarily follow that the values of the suffixes applied to the symbols in a formula express the *absolute* numbers of the atoms of each kind present: thus a molecule of water contains twice as many atoms of hydrogen as it does of oxygen; but whether 2 or  $2n$  atoms of hydrogen are present, is not known. The assumption is, however, made that the suffixes express the absolute numbers of atoms of each kind present. Hence a simple explanation is given of the observed fact termed the Law of Multiple Proportions: the compounds hydrochloric acid, water, ammonia, and marsh-gas consist of molecules containing respectively 1, 2, 3, and 4 atoms of hydrogen united with 1 atom of chlorine, oxygen, nitrogen, and carbon respectively.

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Hence the formula of a compound expresses the nature and number of the constituent atoms present in a molecule of the substance.

27. This hypothesis does not explain why a molecule of free mercury should consist of but 1 atom, while the molecule of oxygen (which is also a bivalent element) contains 2 atoms; nor why a molecule of phosphorus should contain 4, and one of sulphur at low temperatures 6; but it enables us to explain the allotropy of the elements by the assumption that the molecules of the different modifications are made up of different numbers of constituent atoms (thus the molecule of gaseous oxygen contains 2 atoms, of ozone 3, of sulphur at low temperatures 6 atoms, at higher temperatures 2 atoms), and therefore allows of the inference being drawn that under different conditions of pressure and temperature phosphorus vapour might contain only 2 atoms, or even 1 atom in the molecule, or mercury vapour 2 or 4 atoms to the molecule.

28. The phenomena of dissociation are to some extent accounted for by the atomic hypothesis; the atoms in a molecule are held together by certain forces, which are altered in character by variation of temperature; at some given temperature the molecule is in a state of unstable equilibrium; and at a higher temperature the component atoms alter their mutual relations, taking up more stable positions. The observed facts that dissociation always causes a molecule to form less complex molecules, that atomic weight and specific heat of elements are inversely proportional, that compounds are more stable according as more heat is evolved in their production, and that the boiling-points and melting-points are usually higher the more complex the formula, indicate the existence of some general law connecting together the mutual relations of atoms in a molecule and the effects of the force termed heat upon them,—a conclusion strengthened by the analogous connexions between electricity and equivalents, specific refractive energy and formulæ, specific gravity and formulæ, crystalline shape and formulæ, which appear to indicate analogous laws with respect to other forms of force.

The atomic hypothesis alone does not account for these observed facts; it only colligates them together and allows of their being expressed in concise phraseology: thus the law of isomorphism may be stated in the language of the atomic hypothesis thus:—The same numbers of atoms similarly arranged will give rise to the same crystalline form. In order to *explain* the facts, assumptions other than the primary one as to the existence of atoms must be made; thus, to explain Dulong and Petit's law, the assumption must be made that the effect of a



given quantity of heat in raising the temperature of a solid mass containing a given number of elementary atoms all of the same kind is independent of the nature of the atoms. Apart from the circumstance that this assumption is not true (*e. g.*, silicon, carbon, and boron), it affords no "*raison d'être*" for the fact, it gives no clue to the *cause*.

29. The atomic hypothesis gives no clue to the explanation of the remarkable approximate relationships existing between the numerical values of the atomic weights, nor to the remarkable sequence in which those elements at present known follow each other. Exact experiment (Stas) disproves the possibility of all elementary matter being the same, and of our so-called elements being simply variable numbers of primary atoms united together (*i. e.* allotropic modifications); for if so, a common divisor of all the atomic weights would exist. This does not seem to be the case even in members of the same family.

30. The facts referred to by the terms equivalency, valency, radical, replacement, are in perfect harmony with the atomic hypothesis, if this be slightly extended by the definition that *the power of union together of atoms to form a molecule is said to be due to their possession of combining affinities* (Verwandtschaftseigenschaften) *which mutually saturate one another*, univalent atoms being such as ordinarily exhibit but *one* such affinity, bivalent ones *two*, trivalent *three*, quadrivalent *four*, and so on.

A consequence of this definition might be that the *n* affinities of a multivalent atom might not be all of equal character, and thus that two or more isomeric ternary compounds might be produced by the union of two atoms of lower valency with an atom of higher valency, according to the affinities which were saturated by the first two respectively. Thus the atom of oxygen possesses two affinities, each saturated by one affinity of an atom of univalent hydrogen in the molecule water. If, however, one affinity were saturated with hydrogen and the other with chlorine, two isomeric hypochlorous acids might exist differing in that the affinity which is saturated by hydrogen in the one is saturated by chlorine in the other, and *vice versa*. Similarly four methyl chlorides might exist, according as the chlorine atom saturated the 1st, 2nd, 3rd, or 4th affinity of the quadrivalent carbon atoms. No satisfactory evidence of such difference of affinity-value has yet been obtained, several such supposed cases having disappeared on closer examination.

31. A radical, then, is a portion of a molecule, a group of atoms, the affinities of which do not wholly saturate one another, the radical being uni-, bi-, tri-, quadri-, &c. valent, according as 1, 2, 3, 4, . . . affinities are left unsaturated. *Quoad* the series of bodies into the formulæ of each of which the radical

enters, this group of atoms is indivisible, and to it therefore the term *atom* may be extended, although of course in strictness of speech the term thus applied is a misnomer. Thus water, acetic acid, and caustic soda may be regarded as produced by the union of an atom of hydroxyl (OH) with one of hydrogen, acetyl ( $C^2H^3O$ ), and sodium respectively. The term replacement thus acquires a grammatically exact application, one or more atoms of one kind being removed from a molecule and their places filled up by others. The atomic weight or combining number of a radical (compound atom) is therefore the sum of the weights of its component elementary atoms; and its equivalent is the atomic weight divided by the number of unsaturated affinities which it exhibits in the reaction in question.

32. Changes in valency are thus readily explainable by assuming that one or more pairs of affinities either mutually saturate each other or cease to do so. Thus the atom of free mercury (identical with the molecule) exhibits no valency; but when combined with chlorine, bivalency; the two affinities exhibited in the chloride have therefore saturated one another in the free metal. Carbon is quadrivalent; two carbon atoms may unite by mutually saturating 1, 2, or 3 pairs of affinities, thus giving rise to a radical  $C^2$ , which may be sexi-, quadri-, or bivalent accordingly: thus,  $C^2H^6$ ,  $C^2H^4$ ,  $C^2H^2$ . Hence the observed fact that changes in valency almost invariably proceed by even numbers is readily explainable; so also Gerhardt's law of even numbers.

Those few cases where valency does not differ by even numbers (*e. g.* the oxides of nitrogen not considered to be dissociated, the chlorides, oxychlorides, &c. of vanadium and tungsten, &c.) require the assumption that single affinities can sometimes remain, as it were, latent: the precise meaning of this phrase depends on the view taken of the nature of these affinities; but as it can hardly be doubted that they represent the action of a force of some kind, their exertion or transference being always connected with the manifestation or absorption of force of some kind, the anomalous result is arrived at that an atom is capable of exerting ordinarily certain forces which at other times it does not exert,—a proposition somewhat of the same nature as this, that a given substance should sometimes weigh 5 lbs. and sometimes only 4 lbs.

33. The dissected formula of a compound hence represents, on the atomic hypothesis, not merely the existence of certain reactions, but also certain connexions and relationships between particular groups of the constituent atoms. Of course the formula  $CH^3-CO-OH$  does not represent *in space* the relative positions of the compound atoms; it bears to the true position

about the same relationship as the set of symbols



does to the position of the sun and planets at any given instant, the initial letters representing the planets, the order of writing the order of magnitude of their orbits, and the suffixes the number of their moons.

34. The observed fact that the dissected formula of a compound deduced from the majority of its reactions does not necessarily express all the reactions of this body meets with a ready explanation by the atomic hypothesis; of all the possible ways in which a given number of atoms can be connected together under the influence of certain forces, some must be more stable than others. At the moment of the breaking up or other alteration of a molecule whose component atoms are not arranged in the most stable position with respect to the particular set of forces then acting on them, there must be a tendency for the atoms to alter their original position for more stable arrangements, which will therefore be produced to a greater or less extent for the instant; hence the end products of the reaction will be to a greater or less extent those which would have been formed had the original substance been not what it was, but a mixture of the isomerides containing the atoms in these more stable positions.

35. In the foregoing pages the attempt has been made to establish the following points.

(1) Symbolic representations having no connexion with any theory whatever may be employed to designate and refer to a large number of experimental laws and generalizations.

(2) A certain view of the constitution of matter is capable of affording a *raison d'être* for several, but not all, of these laws and generalizations.

It is instructive to compare these views with the ideas of other chemists on this subject. Thus Gerhardt\* says, "Les formules chimiques . . . ne sont pas destinées à représenter l'arrangement des atomes, mais elles ont pour but de rendre évidentes, de la manière la plus simple et la plus exacte, les relations qui rattachent les corps entre eux sous le rapport des transformations." Reasoning further on this, Sir Benjamin Brodie† has elaborated a "method for the investigation, by means of symbols, of the laws of the distribution of weight in chemical change," wherein the truth of the first of the above two propositions is abundantly proved, and that of the second admitted. The distinguished author, however, is of opinion that ordinary symbols (atomic symbols), when used for the construction of a for-

\* *Chimie Organique* (Paris, 1856), t. iv. p. 566.

† *Phil. Trans.* 1866.



mula, "not only permit, but even compel us to regard it from the atomic point of view. We cannot adopt the atomic symbol and at the same time declare ourselves free from the atomic doctrine." The attempt has been made above to show that the *ordinary* symbols do not necessarily involve the atomic hypothesis at all—that, by suitably choosing definitions, the symbol may be employed and yet the mind of the chemist be free from the atomic doctrine.

Wurtz\* defines an atom as "la plus petite quantité d'un élément qui puisse exister dans un corps composé comme masse indivisible par les forces chimiques;" a molecule "est un groupe d'atomes formant la plus petite quantité d'un corps simple ou composé, qui puisse exister à l'état libre, entrer dans une réaction, ou en sortir." These definitions, which are now all but universally accepted, have the inconvenience of allowing the term "atom" to be employed in a twofold sense, which leads to some misapprehension. In one sense the atom is a finite portion of matter of given weight, and hence possessed of dimensions in space, mass, and time denoted by the formula  $LMT^{-2}$ ; this corresponds to the Daltonian sense. In another sense, however, the atom represents not an absolute weight, a given portion of matter, but a proportion or ratio, *i. e.* a pure number possessed of no dimensions in space, mass, or time: thus water is said to be composed of *two* atoms of hydrogen and *one* atom of oxygen, because the numerical ratio of the weights of oxygen and hydrogen in a given quantity of water is that of 2 to 16; while in certain derivatives from water (*e. g.*, caustic soda) the hydrogen and oxygen are found to coexist in the ratio 1 to 16, which is expressed by saying that a molecule of this substance contains but *one* hydrogen atom to one oxygen atom. Using the term in this latter sense, or in one akin to it, Dr. Williamson† states that the "so-called law of multiple proportions has no existence apart from the atomic theory; those who adopt it seem not be aware that they are using the notion of atoms; or else they are shy of mentioning it;" and again that "when one of those who profess to disbelieve in the atomic theory has ascertained by analysis the percentage composition of a compound, and wants to find its formula, he divides the percentage weight of each element by its atomic weight. He seeks for the smallest integral numbers which represent the proportions of atoms, and he attributes to impurity of his sample, or to error of analysis, any deviation from the atomic formula thus obtained. He looks to the reactions of the body for aid in constructing his atomic formula, and controls his analyses by considerations derived from well-established re-

\* *Leçons de Philosophie Chimique* (Paris, 1864), p. 39.

† C. S. J. [2] vol. vii. pp. 339, 340.

actions; but whenever he is led by any of these considerations to a formula which contains a fraction of any atomic weight, he takes a multiple of the formula sufficiently high to be entirely free from such fractions. In no case does he reason on a basis independent of the atomic theory."

From the exposition given above, and employing the term atom in the sense ascribed to it by Dalton, it is evident that the law of multiple proportion is simply the statement of a generalization first made gravimetrically by Dalton, and subsequently extended volumetrically by Laurent and Gerhardt, viz. that, if certain numbers be attributed to each element respectively, the quantitative compositions of compounds are expressible by taking simple multiples of these numbers and comparing the products thus obtained: these numbers being in the ratios of simple multiples or submultiples of the vapour-densities of the elements, the volumetric composition of compound vapours must necessarily be expressed by simple integral numbers—*i. e.* 1 volume of one constituent to 1 of another, 1 to 2, 2 to 3, &c. This generalization is not identical with the hypothesis of the existence of material atoms, advanced to account for the facts summed up in the generalization; and to say that the law has no existence apart from the atomic hypothesis is to give a meaning to the term atom different from that attributed to it by Dalton\*.

Similarly the method of finding the formula of a body from its percentage composition is a simple deduction from Dalton's generalization, but has no necessary connexion with his hypothesis.

The conclusions to be drawn from the foregoing train of thought appear to be as follows:—The salient facts of chemistry may be expressed in words, and more or less completely represented and referred to by the ordinary symbols, the notion of material atoms possessed of dimensions in space, mass, and time being in no way involved in such expressions or representations. This notion of material atoms does not afford an adequate explanation of several facts, more particularly of those where force of some kind is involved as a necessary element of the point in question; and, moreover, the phraseology of the hypothesis of the existence of such atoms is employed by chemists in different senses, in some cases in senses involving notions the contrary of

\* That the term atom is not used by Dr. Williamson in the same sense as that in which Dalton employed it, is evident from his statement (*loc. cit.*) that "whether elementary atoms are in their nature indivisible or whether they are built up of smaller particles is a question upon which I, as a chemist, have no hold whatever; and I may say that in chemistry the question is not raised by any evidence whatever. They may be vortices such as Thomson has spoken of . . . ."

those introduced by the pure hypothesis *per se*. Hence the final conclusion may be drawn that, the hypothesis being at once unnecessary and insufficient, and its language being in practice ambiguous, it is desirable that in expounding the science of chemistry, whether orally or in text-books, the hypothesis and its language should not be made to occupy the prominent and fundamental position they now fill.

XXXI. *Researches on the Electromotive Force in the Contact of Metals, and on the Modification of that Force by Heat.* By E. EDLUND.

[Concluded from p. 223.]

§ 5.

THE metallic combinations the electromotive forces of which have been determined in the preceding pages, were further investigated with respect to their thermoelectric properties. As, with the exception of the platinum-palladium, there were of each combination two pairs of wires, I experimented first on one pair of each combination. Having investigated all these pairs, I passed to the remaining pairs, for the double purpose of controlling the former determinations and of ascertaining whether there were in the pairs of the same combination any slight differences from each other. This was in fact the case with some of them; and these were submitted to a new trial, effected on each pair. The experiments were made in the following manner:—Near the point of soldering I bent each wire at a right angle, so that the two wires were parallel, and the distance between them 10 millims. The point of soldering was in the middle of the elbow which united them. As the bismuth wire could not be bent, the copper wire to which it was soldered was bent twice at a right angle near the soldering, which, therefore, was near the elbow instead of in the middle of it. The wires thus prepared were passed through the cork into a large test-glass. Through the same cork a very sensitive thermometer was so placed that its little cylindrical bulb rested with its middle against the point of soldering. The test-glass was introduced through an aperture in the centre of a thin wooden lid fitted to a large glass vessel filled with cold water. In order that the warming of the water by the air of the room might be very slow, the glass vessel was surrounded by a layer of cotton wadding. The free ends of the wires, issuing from the cork in a vertical direction, having passed through the bottom of a small wooden box, were there united by small screws to the conducting wires of the magnetometer. In the wooden box another thermometer, exactly resembling the preceding, was placed with its bulb quite close to the



points of union. The holes in the bottom of the box and in its upper surface were closed with cotton, to prevent the entrance of the external air, which would have rapidly altered the temperature; this was, besides, nearly the same as that of the external air, but changed more slowly. During the whole of the experiments the temperature of the point of soldering was about  $+10^{\circ}$ . To investigate the galvanic conductivity, a bobbin covered with copper wire had been placed in the circuit. A steel magnet, placed in the coil, had sufficient space to be moved a certain distance. The inductive force thus remaining invariable, the deviations of the magnetometer obtained through the inductor would be proportional to the conducting-power. A rheostat had also been placed in the circuit; but it was only required for the bismuth-copper combination, as the latter gave deviations several times as great as the others. The magnetometer, the deviations of which were read in the usual manner (with the aid of a telescope and a scale), had a perfectly astatic system of needles, so that the earth's magnetism exerted no influence upon it. The necessary directing force was obtained by suspending the system, with its mirror, from a thin silver wire, the torsion-force of which gave to the system a determined direction. This mode of suspension has the important advantage of being totally independent of the variations of the terrestrial magnetic declination. The deviations are proportional to the intensity of the current. When a fresh pair of wires had been introduced, the first observation was not taken until the points of contact had acquired the temperature indicated by the thermometer belonging thereto, after which the observations succeeded one another at short intervals. With this method of observation, there was perhaps reason to fear that some heat would pass from the warmest to the coolest parts of the wires, and that consequently the points of contact would not have exactly the temperature indicated by the thermometers, the indications of which depended partly also on the heat of the surrounding air. In order to see if this was the case, the wires of a pair of combinations were joined to wires of the same sort, so that the distance, from the wooden box above mentioned, to the cork of the test-glass was more than doubled; but the deviation remained unaltered. The presumed possibility of errors in this respect had therefore no existence. In the results of experiments given below, all the observations relating to the same combination have been combined into a single group, although they may not have been made at the same time.

*Copper-Tin.*

Exp. 41.	No. 1.	
Difference of temperature at the points of contact.	Deviations of the magnetometer.	Conducting-power of the circuit. (Deviations with the inductor.)
8.7 . . .	35.0	166.0
8.6 . . .	34.0	166.0
8.4 . . .	32.4	Mean . 166.0
8.4 . . .	32.5	
8.4	Mean . . 33.48	
Mean . 8.5		

Exp. 42.	No. 2.	
7.9 . . .	33.2	167.5
7.7 . . .	32.0	167.5
7.6 . . .	31.9	Mean . 167.5
7.5 . . .	30.6	
Mean . 7.68	Mean . 31.93	

If from the deviations of these two experiments we calculate what they would have been with a difference of temperature of  $10^{\circ}$  and the conductivity of the circuit = 160, we obtain,

For the 1st combination, dev. = 37.96
„ 2nd „ „ = 39.72
Mean . . = 38.84

*Copper-Silver.*

Exp. 43.	No. 1.	
Diff. of temp.	Deviations.	Conducting-power.
9.0 . . .	1.2	166.0
8.4 . . .	1.2	167.0
8.3 . . .	1.2	167.0
8.1 . . .	1.2	Mean . 166.7
Mean . 8.45	Mean . 1.2	

Exp. 44.	No. 2.	
8.8 . . .	2.2	168.0
8.7 . . .	2.2	168.0
8.2 . . .	2.1	Mean . 168.0
8.1 . . .	2.1	
Mean . 8.45	Mean . 2.15	

The calculation of the deviation for these two experiments gives:—

No. 1. Deviation	= 1.36
No. 2. „	= 2.42
Mean . .	= 1.89

*Copper-Aluminium.*

Exp. 45.	No. 1.		
Diff. of temp.	Deviations.	Conducting-power.	
7.4 . . . .	32.0	167.0	
7.2 . . . .	31.0	167.0	
7.3 . . . .	31.5	Mean .	167.0
7.3 . . . .	31.0		
Mean . 7.3	Mean . 31.38		

Exp. 46.	No. 2.		
8.1 . . . .	37.3	168.0	
7.9 . . . .	36.7	168.0	
7.9 . . . .	35.0	Mean .	168.0
7.8 . . . .	34.6		
Mean . 7.93	Mean . 35.9		

Whence

No. 1. Deviation	= 41.18
No. 2. „	= 43.12
Mean . . .	= 42.15

*Cadmium-Copper.*

Exp. 47.	No. 1.		
Diff. of temp.	Deviations.	Conducting-power.	
9.6 . . . .	10.5	167.0	
9.5 . . . .	10.1	166.5	
9.3 . . . .	10.0	Mean .	166.8
9.1 . . . .	9.8		
8.9 . . . .	9.7		
Mean . 9.28	Mean . 10.02		

Exp. 48.	No. 1.		
10.4 . . . .	10.5	166.0	
10.3 . . . .	10.0		
10.4 . . . .	10.2		
10.2 . . . .	10.2		
9.9 . . . .	10.5		
Mean . 10.21	Mean . 10.28		



Exp. 49.	No. 2.	
Diff. of temp.	Deviations.	Conducting-power.
8·9 . . .	9·0	167·5
8·6 . . .	8·2	167·5
8·2 . . .	8·3	Mean . 167·5
8·1 . . .	7·5	
Mean . 8·45	Mean . 8·25	

Whence we obtain:—

No. 1. Deviation	= 10·36
„ „	= 9·68
No. 2. „	= 9·33
Mean .	9·79

*Copper-Platinum.*

Exp. 50.	No. 1.	
Diff. of temp.	Deviations.	Conducting-power.
8·6 . . .	51·0	166·0
8·6 . . .	50·5	163·5
8·5 . . .	50·5	Mean . 164·8
8·3 . . .	50·0	
Mean . 8·5	Mean . 50·5	

Exp. 51.	No. 2.	
7·8 . . .	47·0	168·0
7·4 . . .	47·0	168·0
7·3 . . .	45·5	Mean . 168·0
7·2 . . .	45·0	
Mean . 7·53	Mean . 46·13	

Whence we obtain:—

No. 1. Deviation	= 57·68
No. 2. „	= 59·13
Mean . .	= 58·41

*Copper-Gold.*

Exp. 52.	No. 1.	
Diff. of temp.	Deviations.	Conducting-power.
8·7 . . .	21·5	166·0
8·4 . . .	20·0	
8·0 . . .	19·3	
7·9 . . .	20·3	
Mean . 8·25	Mean . 20·28	

Exp. 53.	No. 2.		
Diff. of temp.	Deviations.	Conducting-power.	
8.8 . . .	22.0	167.0	
8.4 . . .	21.5	167.0	
8.3 . . .	20.5	Mean . 167.0	
8.2 . . .	21.0		
Mean . 8.43	Mean . 21.25		

Whence we obtain :—

No. 1. Deviation	= 23.69
No. 2. „	= 24.15
Mean . .	= 23.92

*Iron-Copper.*

Exp. 54.	No. 1.		
Diff. of temp.	Deviations.	Conducting-power.	
6.0 . . .	88.2	164.5	
6.0 . . .	84.0	166.0	
5.8 . . .	84.8	Mean . 165.3	
5.7 . . .	82.5		
5.7	Mean . 84.88		
Mean . 5.84			

Exp. 55.	No. 1.		
7.9 . . .	119.5	166.0	
7.7 . . .	117.5	167.0	
7.7 . . .	117.0	Mean . 166.5	
7.4 . . .	116.3		
Mean . 7.68	Mean . 117.8		

Exp. 56.	No. 2.		
8.3 . . .	126.4	166.0	
8.4 . . .	127.0	166.0	
8.3 . . .	127.8	Mean . 166.0	
8.3 . . .	127.0		
Mean . 8.33	Mean . 127.05		

Exp. 57.	No. 2.		
7.8 . . .	121.5	165.0	
7.6 . . .	117.5	166.7	
7.5 . . .	115.5	166.0	
7.4 . . .	115.0	Mean . 165.7	
Mean . 7.58	Mean . 117.4		

Whence we obtain:—

No. 1.	Deviation	=140·7
"	"	=147·4
No. 2.	"	=147·0
"	"	=149·6
Mean	.	=146·18

*Copper-Lead.*

Exp. 58.	No. 1.		
	Diff. of. temp.	Deviations.	Conducting-power.
	5·7 . . .	17·0	167·0
	5·8 . . .	17·0	167·0
	5·8 . . .	17·0	Mean . 167·0
	5·9 . . .	17·0	
Mean .	5·8	Mean .	17·0
Exp. 59.	No. 1.		
	9·0 . . .	24·5	166·5
	8·8 . . .	24·0	168·0
	8·5 . . .	23·0	Mean . 167·3
	8·4 . . .	23·8	
Mean .	8·68	Mean .	23·83
Exp. 60.	No. 2.		
	8·4 . . .	26·1	166·5
	8·0 . . .	24·8	165·5
	7·9 . . .	24·5	Mean . 166·0
	7·7 . . .	23·5	
Mean .	8·0	Mean .	24·73
Exp. 61.	No. 2.		
	6·7 . . .	17·0	166·0
	6·5 . . .	17·0	167·0
	6·4 . . .	16·9	Mean . 166·5
	6·2 . . .	16·0	
Mean .	6·45	Mean .	16·73

Whence we obtain:—

No. 1.	Deviation	=28·08	} 27·17
"	"	=26·26	
No. 2.	Deviation	=29·79	} 27·36
"	"	=24·93	
Mean	.	.	27·27



Copper-Bismuth.

Exp. 62.	No. 1.		
	Diff. of temp.	Deviations.	Conducting-power.
	8.3 . . .	297.0	64.5
	8.1 . . .	288.8	64.0
	7.9 . . .	282.5	64.5
	7.7 . . .	276.0	64.5
Mean .	8.0	Mean . 286.08	Mean . 64.4

Exp. 63.	No. 1.		
	7.9 . . .	285.0	64.5
	7.9 . . .	275.0	63.0
	7.7 . . .	268.0	65.0
	7.5 . . .	258.7	Mean . 64.2
Mean .	7.75	Mean . 271.7	

Exp. 64.	No. 2.		
	9.1 . . .	290.0	64.5
	8.8 . . .	286.0	65.5
	8.7 . . .	282.0	64.5
	8.4 . . .	276.0	66.5
Mean .	8.75	Mean . 283.5	Mean . 65.3

Exp. 65.	No. 2.		
	6.8 . . .	216.3	65.3
	6.6 . . .	217.0	
	6.8 . . .	215.0	
	6.8 . . .	216.0	
Mean .	6.75	Mean . 216.08	

Whence we obtain :—

No. 1.	Deviation	=888.4	} 881.05
"	"	=873.7	
No. 2.	"	=793.9	} 789.15
"	"	=784.4	
Mean . .			835.10

The great thermoelectric difference between the two bismuth-copper combinations is remarkable. Doubtless the cause of it is the crystalline constitution of bismuth. It is known that the thermoelectric force of bismuth and of antimony varies with the plane of crystallization: the crystals which at the point of junction are in contact with the copper may have different positions in the two combinations.

*Zinc-Silver.*

Exp. 66.	No. 1.	
Diff. of temp.	Deviations.	Conducting-power.
10.0 . . .	3.2	165.5
9.9 . . .	3.0	165.5
9.7 . . .	3.0	Mean . 165.5
9.5 . . .	3.0	
Mean . 9.78	Mean . 3.05	

Exp. 67.	No. 2.	
7.6 . . .	2.0	168.0
7.4 . . .	1.8	168.7
7.2 . . .	1.5	168.2
7.1 . . .	1.7	Mean . 168.3
Mean . 7.33	Mean . 1.75	

Whence we obtain:—

No. 1. Deviation	= 3.02
No. 2. „	= 2.27
Mean . .	= 2.65

*Platinum-Palladium.*

Exp. 68.		
Diff. of temp.	Deviations.	Conducting-power.
7.3 . . .	41.8	166.0
7.3 . . .	43.0	166.0
7.1 . . .	42.6	Mean . 166.0
7.3 . . .	43.0	
Mean . 7.25	Mean . 42.6	

Whence we obtain:—

$$\text{Deviation} = 56.63.$$

In each combination the thermoelectric current passed, through the warmer point of contact, from the second to the first metal of the combination, consequently from copper to iron, from bismuth to copper, &c. We thus obtain, for the combinations investigated, the following thermoelectric series, in which the numbers designate the thermoelectric force arising from the contact with copper:—

+ Iron . .	146·18	Lead . .	27·27
Cadmium .	9·79	Tin . .	38·84
Zinc . .	0·76	Aluminium	42·15
Copper . .	0·00	Platinum .	58·41
Silver . .	1·89	Palladium	115·04
Gold . .	23·92	— Bismuth .	835·10

The metals, therefore, keep the same order in the electromotive as in the thermoelectric series.

### § 6.

If we compare the electromotive series, as determined in the preceding pages, with the electric-tension series, as given by Volta, Pfaff, Péclet, and others, it is impossible to find the least concordance between them. Thus, for example, according to Volta's series, zinc is positive to iron, while it is the opposite according to the series above given; according to the tension-series, bismuth is positive to platinum, while in the above series bismuth is found far below platinum in the negative direction; according to the tension-series, lead is much more positive than copper, while it is the contrary in the series determined by me, &c. Yet the cause of this want of concordance is now not difficult to discover. Grove's gas pile and galvanic polarization are proofs that gases are electromotive in contact with solid bodies. I have, I think, demonstrated in a former paper\* that galvanic polarization produces a veritable electromotive force by the contact of the gases precipitated on the polar surfaces. The polarization-current produced cannot be regarded as proceeding from chemical activity in the polarization-vessel, but has its true cause in the polar plates being covered by the precipitated gases. The difference, therefore, between the two series results from the fact that gases are electromotive in contact with solid bodies. The experiments on which the tension-series is founded were made in free air. If, then, with the aid of the electroscope we investigate the electrical state of a disk composed, for example, of copper and zinc, we have to do not merely with the mutual contact of the two metals, but also the contact of both with the surrounding air. As solid bodies have the property of more or less condensing gases and retaining them at their surfaces, the same result is in general obtained when the experiment takes place in a space in which the air is rarefied, or in a vacuum, since this does not entirely remove the gas from the surface of the solid. In the experiment, therefore, we have to take into consideration three electromotive contacts. The deviation given by the electro-

\* *Öfversigt af K. Vet.-Akad. Förh.* 1867, p. 95. *Pogg. Ann.* vol. cxxxi. p. 386. *Phil. Mag.* S. 4. vol. xxxv. p. 103.



scope is a measure of the resultant of these three forces ; it is therefore not a result of the metallic contact exclusively, but of all three combined ; and we ought not to be surprised that the two series do not mutually agree. It is not a new fact that gases condensed at the surface of metals exercise an influence on electroscopic experiments. As I have mentioned above, this fact has been urged as an objection to the correctness of the contact theory. But the amount of this influence only became fully evident when the true electromotive series was established as it comes out from metallic contact only.

From the preceding we are fully authorized to formulate the following propositions :—

1. *As established by electroscopic experiments, the electric-tension series presents no immediate relation with the electromotive forces at the contact of metals ; therefore it is impossible to determine from that series the amount or the nature of those forces.*

The following proposition is an immediate consequence of the preceding :—

2. *The order of the metals is perfectly identical in the electromotive and in the thermoelectric series respectively.*

The identity of the two series indicates an intimate connexion between the electromotive and thermoelectric forces. The electromotive forces of contact transform heat into electricity. At the absolute zero of heat, supposing that point to have an actual existence, it would be impossible for those forces to produce electric motion. In this circumstance we arrive spontaneously at the supposition that the power of these forces to occasion an electric motion depends on the quantity of heat present, or, in other words, that it is a function of the temperature. In fact Le Roux has verified this by experiment. He found that the quantity of heat produced or absorbed when a galvanic current circulates through the point of contact between bismuth and copper is greater at the temperature of  $+100^{\circ}$  than when the experiment takes place at the ordinary temperature. The electromotive force resulting from the contact between bismuth and copper is therefore greater at the former temperature than at the latter. Let us suppose several metals A, B, C, &c. soldered to one another in a ring : the sum of the electromotive forces will be equal to 0 when the temperature is the same at all the soldering-points ; if, on the contrary, the temperature be augmented at one of these points, a thermoelectric current will result. This comes from the electromotive force having been modified by the increase of temperature. Thermoelectric currents therefore constitute a measure of the modification undergone by the electromotive force when the temperature is raised or lowered.

In the experiments related above it was shown that, at the

warmer point of contact, the thermoelectric current always followed the same direction as the current produced by the electromotive force of the point of contact. This force was consequently more intense at the warmer than at the cooler point of contact; or, in other terms, the electromotive force of contact increased with the temperature. These experiments took place between limits of temperature of about +10 and +20 degrees. In the experiments executed for the purpose of ascertaining the change of temperature at the point of contact on the passage of a current, the temperature of the wires did not exceed +30 degrees, and in most cases was much lower. We are therefore authorized to formulate the following proposition:—

3. *The electromotive force of contact, for the eleven metallic combinations investigated, increased with the temperature when the experiments took place at a temperature not exceeding +30 degrees.*

That this proposition cannot be applied to all temperatures results from the researches, on thermoelectric phenomena, which several men of science have executed at higher temperatures. From these it is known that the thermoelectric current may diminish with the increase of the temperature at the points of contact. From the numbers given for the two series, it follows that the thermoelectric current is greater for the metallic combinations whose electromotive force is greater than for those possessing an inferior electromotive force. The exact ratio between the electromotive forces and the corresponding thermoelectric currents is obtained by dividing the numbers of one of the series by the corresponding numbers of the other. The following Table gives the quotients obtained by dividing the numbers designating the amounts of the electromotive force by those representing the magnitudes of the corresponding thermoelectric currents:—

Iron-copper . .	1·12	Copper-tin . .	1·57
Cadmium-copper	1·42	„ -aluminium	1·37
Zinc-copper . .	2·24	„ -platinum	1·30
Copper-silver . .	1·47	„ -palladium	1·20
„ -gold . .	1·62	„ -bismuth	1·97
„ -lead . .	1·23		

Relying on theory, one would have expected that these quotients would be equal in amount, or (which comes to the same thing) that the electromotive and thermoelectric forces would be proportional to each other; but this is far from being the case. As a rule, the quotients diminish in proportion as the electromotive forces increase. It is, in my opinion, certain that this departure from theory cannot depend on possible errors of observation. Doubtless these errors, in the deter-

mination of the electromotive forces, may be pretty large; nor is this very astonishing, the differences of temperature to be measured being so very small. In the case of the zinc-copper combination, for example, the difference does not amount to one thousandth of a degree; in that of the cadmium-copper, hardly to one and a half hundredth; and for copper-bismuth (which, of all the combinations, possesses the greatest electromotive force), to 1.5 degree. But the errors of observation are, after all, in no case sufficiently great to furnish the explanation of the considerable variations in the quotients above given. Let us compare, for example, the copper-gold with the iron-copper combination. A series, consisting of observations made with three different intensities of current, gave, almost without variation, the number 14.5 for the electromotive force of the former combination; another series, in which the observations followed one another at 15-minute intervals, gave 15.02. The mean, 14.76, cannot be singularly faulty. The electromotive force of the iron-copper combination was determined in the same manner by means of several series, which gave, as the mean, 130.99, in which the probable error cannot be very great.

The same two combinations had also been investigated by using two cylinders the external surface of which was not silvered; and I obtained 12.56 for copper-gold, 115.73 for iron-copper. The ratio between the two former numbers does not much differ from that between the two latter. It is the same with most of the other combinations. The only ones which can betray a greater uncertainty are the zinc-copper and copper-silver combinations, in which the differences of temperature were very insignificant. We can therefore formulate the following proposition:—

4. *The thermoelectric forces which, at a given difference of temperature, arise in different metallic combinations, are not proportional to the electromotive forces of those same metallic combinations.*

By the application of the second fundamental principle of the mechanical theory of heat, Clausius has demonstrated that the augmentation undergone by the electromotive force when the temperature rises at the point of contact should be proportional both to the augmentation of temperature and to the electromotive force itself. If Carnot's function be made equal to  $A(a+t)$ , in which  $A$  is the equivalent of heat for the unit of work,  $t$  the temperature in degrees Celsius, and  $a$  the number 273, it follows from this deduction that  $E=e(a+t)$ , in which  $E$  is the electromotive force at the temperature  $t$ , and  $e$  a constant dependent exclusively on the nature of the metals forming the contact. It would hence follow that all the quotients above given should be of equal amount, which nevertheless experiment has



shown not to be the case. Another result of the theory is, that the thermoelectric currents should be, whatever the temperature, proportional to the difference of temperature between the points of contact—which, as we know, agrees no more than the former with the practical results. Now the cause of this may indeed be, as Clausius assumes, that at high temperatures metals undergo a molecular modification, the thermoelectric effect of which cannot be taken into account in the calculation. It is, however, infinitely more difficult to explain why the quotients above mentioned are not identical, as the theory requires; here we have neither high temperatures nor sensible molecular modifications in the metals. I cannot omit to call attention to a result I obtained some years since, during researches on the calorific phenomena arising from change of volume in solid bodies\*. If a wire be stretched, it cools; and if it then be permitted to contract slowly without the particles beginning to oscillate, it becomes warmer by a quantity equal to the cooling effected by the tension. Thomson has calculated these changes of temperature by means of the second axiom of the mechanical theory of heat. Now, on comparing the results of experiment with the theoretical calculation, they are found not to agree. As is known, the mechanical equivalent of heat enters into Thomson's formula; and it is necessary to give that equivalent the value of 683 kilogrammetres in order to make the result of the experiments agree with the theory. The concordance between the series of experiments shows that this number cannot be more than a few per cent. wrong; and this is also confirmed by the circumstance that, if the stretched wire be suffered to contract suddenly and without accomplishing any external mechanical work, the resulting quantity of heat, calculated with the aid of the number 683 already found, gives the correct value of its mechanical equivalent, viz. 434 kilogrammetres. It has been attempted to explain the want of agreement between the theory and my experiments by the formula presupposing that the body undergoes, under the influence of heat, an equal dilatation in all directions, which, perhaps, is not the case with stretched wires like those employed in my experiments†. But in a recent paper‡, M. Dahlander has demonstrated the invalidity of the explanation. As we have no right to reject a fact because it is opposed to those which we already know, I conclude by formulating the following proposition as a further result of my experiments:—

\* *Öfversigt af K. Vet.-Akad. Förhandl. för 1865*, p. 95. *Pogg. Ann.* vol. cxxvi. p. 589. *Ann. de Chim. et de Phys.* ser. 4. vol. viii. p. 257.

† Paul de Saint-Robert, *Atti della Reale Accad. delle Scienze di Torino*, Jan. 1868; *Ann. de Chim. et de Phys.* ser. 4. vol. xiv.

‡ *Öfversigt af K. Vet.-Akad. Förhandl. för 1871*.

5. If, with the aid of the second fundamental principle of the mechanical theory of heat, we calculate the modifications undergone by the electromotive forces of contact in consequence of the increase of temperature, we obtain results which do not agree with experiment.

XXXII. *Acoustical Experiments showing that the Translation of a Vibrating Body causes it to give a Wave-length differing from that produced by the same Vibrating Body when stationary.*  
By ALFRED M. MAYER, Ph.D., Professor of Physics in the Stevens Institute of Technology, Hoboken, New Jersey, U.S. America\*.

*The Apparatus.*

FOUR tuning-forks mounted on resonant cases and giving the note  $UT_3$ , = 256 complete vibrations per second, were obtained. I will designate them as Nos. 1, 2, 3, and 4.

Nos. 1 and 2 were brought into perfect unison by a process to be described.

No. 1 was placed before a lantern; and just touching one of its prongs was a small ball (6 millims. diameter) of good cork, suspended by a silk fibre. The images of the fork and of the cork ball were projected on a screen.

No. 3 had one prong weighted with wax, so that it gave 2 beats a second with No. 1 or 2.

No. 4 had the ends of its prongs filed off until it also gave 2 beats a second with No. 1 or 2; thus No. 4 gave 2 vibrations a second more than No. 1, while fork 3 gave 2 vibrations per second less than No. 1.

*The Experiments.*

In the experiments 1 to 7 inclusive, No. 1 remains before the lantern with the suspended cork ball just touching one of its prongs.

*Exp. 1.*—Fork No. 2, screwed on its case, was held in the hand at distances from 30 to 60 feet from No. 1 and sounded; the ball was projected from the prong of fork 1, which vibrates in unison with No. 2.

*Exp. 2.*—I stationed myself 30 feet distant from fork 1, and fork No. 2 was screwed off its case and vibrated in one hand while the case was held in the other. I now walked rapidly towards fork no. 1; and after I was in regular motion I placed the fork on its case, and just before I ceased walking I took it

\* Communicated by the Author.

off. Although when I did so I was only about a foot from fork 1, yet the cork ball remained at rest against its prong.

*Exp. 3.*—Again I walked towards 1 as in experiment 2, but I did not remove the fork from its case after it was placed on it. The ball remained at rest until the moment I suddenly stopped walking; but at that instant the ball flew from the fork, while an assistant, whose ear was close to the case of fork 1, while his eye was directed to the screen, found that at the instant I stopped walking the fork No. 1 sounded, while the ball jumped from its prong.

*Exps. 4 and 5.*—These experiments were exactly like 2 and 3, except that I walked away from fork 1 instead of approaching it. The results were the same as in *exps. 2 and 3.*

*Exp. 6.*—Fork No. 3, giving 254 vibrations per second, was sounded as in *exp. 1.* It had no effect in moving the ball. I now screwed the fork off its case, and, standing about thirty feet from fork 1, with my arm I swung the case towards fork 1, and while it was approaching it I placed fork No. 3 on its case; the proper velocity (from 8 to 9 feet per second) having been obtained, the ball was suddenly projected from fork 1. On greatly increasing or decreasing the above velocity of the moving case, the vibration of fork 3 produced no effect on fork 1.

*Exp. 7.*—Fork No. 4, which gave two vibrations a second more than No. 1, was substituted in *exp. 6,* but was placed on its swinging case when this was *receding* from fork 1. The effect of this motion and of varying velocities was the same as in *exp. 6.*

*Exp. 8.*—I placed fork 3 before the lantern, and swung fork 1 as in *exp. 7;* the effects were the same as described in *exp. 7.*

*Exp. 9.*—I now placed fork 4 before the lantern and moved fork 1 as fork 3 was moved in *exp. 6.* The effects on the ball were the same as in *exp. 6.*

These are the simple means I have arrived at to show the change of wave-length produced by the translation of the vibrating body. By analogy they clearly unfold that exquisite modern method of determining the motions of a heavenly body by variations in the refrangibility of the rays which it emits—motions often impossible even to detect by any other means. I therefore deem it proper that I should proceed to state the delicate conditions on which depend the perfection of experiments which so satisfactorily elucidate the nature of those grand and refined problems offered to spectral observation, while they afford an experimental proof of the important theorem that Doppler established in 1841.

It is first of all essential that forks 1 and 2 should really be in unison. Two forks sounded together may give no perceptible



beats; or they may constrain each other into a common forced oscillation; and thus both will give the same number of vibrations, yet may be removed from equality when separately sounded. The process I have adopted is as follows:—Three forks are taken that are supposed to make the same number of vibrations in a given time. They are supported on india-rubber tubing and are thus insulated. One of the forks is now loaded, so that it gives two or three beats in a second with one of the other two that are to be brought into exact unison. The interval of time occupied by twenty or thirty of these beats is accurately measured by means of a chronograph (one of Casella's registering stop-watches does well). The interval occupied by the same number of beats given by the second fork is now ascertained; and if it differs from that given by the first, the quicker vibrating fork is made to give the same number of beats as the slower by loading it with wax. When the forks have thus been carefully adjusted, I have had no difficulty in projecting the ball in exp. 1 at a distance of 60 feet; and I believe that it could have been accomplished at a distance of 100 feet.

The ball of cork should be *spherical*, so that it will always just touch the fork, no matter how it may rotate around its suspending thread, which latter should consist of only one or two fibres of unspun silk. The cork is rendered as smooth as possible, and is then *varnished*. This is important; for the varnish gives a firm coating to the ball without sensibly increasing its weight, and is especially useful in covering the minute asperities or elastic projections on its surface, which otherwise would act as "buffers" to the impacts of the fork and deaden its projectile effects.

The above-stated conditions having been attained, no physicist will have any difficulty in repeating these experiments.

A machine has been devised by which a uniform motion of translation can be given to the forks; and with this I propose making a quantitative investigation of the phenomena, using an apparatus essentially the same in its action as the one here described.

We may substitute for the suspended cork ball a light plane mirror held between two stretched vertical fibres while one of its edges just touches the fork. The motions of a beam of light reflected from the mirror to a screen indicate most beautifully the vibrations of the fork. This ingenious and most delicate arrangement for indicating vibrations is due to Professor O. N. Rood, of Columbia College, New York, who first used it in a public lecture delivered in New York city on the 28th of last December. We have, however, in our special work found the image of the projected ball more convenient and sufficiently delicate for our experiments.

*Quantitative Relations in the experiments and analogical facts in the phenomena of Light.*

The UT<sub>3</sub> No. 1 fork makes 256 complete vibrations in one second, while fork No. 3 makes 254, giving for the respective wave-lengths of these vibrations 4·367 and 4·401 feet, which we will designate in order as  $\lambda$  and  $\lambda'$ : we will take 1118 feet per second as the velocity of sound at 60° Fahr.

Now

256 vibrations in 1118 feet make . . . .  $\lambda = 4·367$  feet,  
and

254 vibrations in 1118— $2\lambda$  ( $= 1109·266$ ) make  $\lambda = 4·367$  feet.

As the velocity of propagation of the vibrations and  $\lambda$  are the same in both cases, it follows that  $\left(n = \frac{V}{\lambda}\right)$ , the number of vibrations in a second reaching a distant point, is the same, and therefore 256 vibrations from a body at rest will produce the same effect on a distant surface as 254 vibrations emanating from a body which moves towards that surface with a velocity of  $2\lambda$  or of 8·734 feet per second; and this is the velocity we gave the forks in exps. 6 to 9.

We will now examine analogical phenomena in the case of light. Fork 1 giving 256 vibrations a second, let these represent 595 million million vibrations a second, which we will take as the number of vibrations made by the ray D<sub>1</sub> of the spectrum. Then fork No. 3 will represent 590 million million vibrations per second, which give a wave-length ·0000042 millimetre longer than that of D<sub>1</sub>, and nearly correspond with an iron line situate ·42 division below D<sub>1</sub> on Angström's chart. We saw that fork No. 3, giving 254 vibrations a second, had to move towards the ear with a velocity of 8·734 feet to give the note produced by 256 vibrations a second emanating from a fixed point; so a star sending forth the ray which vibrates 590 million million times a second, will have to move towards the eye with a velocity of 28,740 miles per second to give the colour produced when ray D<sub>1</sub> emanates from a stationary flame.

February 8, 1872.

XXXIII. *On the Heat-Spectrum of the Sun and the Lime-Light.*  
 By M. S. LAMANSKY of St. Petersburg\*.

IN these investigations, which I carried out in Geh. Rath Helmholtz's laboratory at Heidelberg, I endeavoured to analyze by means of a prism as large a cone of light as possible. For this purpose the following arrangement was hit upon.

Solar rays, reflected from the mirror of a heliostat, were collected by means of a lens of 3 inches aperture and 25 inches focal distance. Into the focus of the lens a slit was brought, the length of which was exactly equal to the diameter of the sun's image. The issuing luminous pencil was analyzed by a flint-glass prism of 2 inches aperture and a refracting angle of  $60^\circ$ . The separated rays were collected by an achromatic lens. This lens was placed at twice its focal distance both from the slit and from the linear thermo-apparatus. The latter consisted of 12 pairs of bismuth-antimony elements, and was connected with a thermo-multiplier after Magnus. With this arrangement the luminous rays covered the entire face of the prism; and in the investigation of the ultra-red rays the prism was placed at the minimum of deflection for the red.

Hence resulted a very pure spectrum, the investigation of which was conducted in the following manner. First the double slit of the thermo-apparatus, of only half a millim. breadth, was placed upon the line D; the thermo-apparatus, enclosed in a tin-plate box containing hot water for compensation of the temperature, was then moved along a scale of millimetres in order to trace, step by step, the distribution of heat in the entire spectrum. In subsequent experiments, which I executed in the past summer with rock-salt apparatus, a micrometer-screw was added, suitable for shifting the thermo-apparatus, which, moreover, stood behind the slit of a thick plate of brass. Before each observation the solar image was first thrown upon the slit by the mirror of the heliostat; the screen between the slit and the prism was then removed, and the deflection of the magnet read off with telescope and scale. I usually made two observations for each position of the thermo-apparatus in the spectrum, and took their mean. Finally, however, I came back again to the line D; in this way I was able to ascertain the variation of intensity of the heat-effect during the experiment.

I. *Distribution of the Heat in the Solar Spectrum.*

In the above-described manner I investigated the thermic effects of the solar spectrum with flint-glass prisms, with prisms

\* Translated from a separate copy, communicated by the Author, from the *Monatsberichte der Königl. Akademie der Wissenschaften zu Berlin*, Dec. 7, 1871.



of sulphide of carbon, and with rock-salt apparatus. I will here mention that these apparatus were of the same dimensions as the above-mentioned glass ones, were of perfectly transparent Stassfurth salt, and excellently made by W. Steeg, optician, of Homburg; and I fresh-polished them before each experiment. My experiments have reference to the forenoons in the summer and autumn of the past and the present year, and were only made with a cloudless sky, as the slightest cloud occasioned a marked difference in the deflections.

In all such experiments, if our observations, commencing at the line D, advance to the ultra-red end of the spectrum, the deflections, answering to the heat-effects, become gradually, but not proportionately, stronger, until they attain a certain maximum, and then diminish; and this takes place four times. We therefore see here a discontinuous distribution of heat in the solar spectrum; namely, the ultra-red rays are interrupted in three places by breaks or bands.

This want of continuity was proved by Sir John Herschel in the following manner (Phil. Trans. 1840). By means of a flint-glass prism he threw a spectrum upon paper blackened with soot and moistened with alcohol; and by the time of drying he determined the thermal effect of the spectrum. He at the same time observed that the moistened surface dried in a series of four distinctly separate spots. Herschel's chief concern, however, was merely to ascertain the conditions under which these four spots made their appearance. He observed that in the spectrum of a crown-glass prism they were less distinctly separated from each other, and that in the investigation of the solar spectrum with a water prism they were only feebly expressed.

The existence of such bands in the ultra-red rays was afterwards noticed by Fizeau and Foucault (*Comptes Rendus*, vol. xxv.), in their well-known experiments on the interference of the heat-rays. I know of no other observations on these bands in the solar spectrum. At least, all the philosophers who have hitherto investigated the distribution of heat in the solar spectrum with glass prisms, as well as with rock-salt apparatus, make no mention of them, and still as previously delineate the heat-curve as continuous.

These bands can be distinctly observed with all three of the above-mentioned prisms; only the spectrum must be perfectly pure.

They have a corresponding position in the spectra of all three prisms, and only differ by being broader when the prism used has greater dispersive power, as sulphide of carbon, than when it has less, as rock salt.

These three breaks or bands are not of equal breadth; the

first is much more sharply separated from the second than the second from the third. It may easily happen, if the movement of the thermo-apparatus be not sufficiently delicate, that the second and third appear as one common broad break.

Nevertheless we can only obtain a correct notion of the nature of these bands from experiments with rock-salt apparatus, because the ultra-red rays are strongly absorbed by glass. At the end of August and in September last, when we here had very fine, hot, sunny days, I made many experiments on this point. At that time I could examine the heat-spectrum of the sun between 7 A.M. and 1 P.M.; and I took care to make two parallel experiments in one forenoon—one in the morning, and the other about noon. In each such experiment I traced the thermal effects from the line D to the ultra-red end, to where the heat-effect entirely ceased or had become very feeble.

These parallel experiments showed that the breaks become somewhat narrower with increasing altitude of the sun. They were rather deeper on the days when the relative moisture of the air was greater. But the observations cited are at all events not sufficient to permit us at once to explain them as atmospheric lines, especially when we take into consideration that the apparent magnitude of these breaks may possibly have depended on the variations of intensity of the ultra-red rays at different hours of the forenoon. In order, therefore, to decide the question whether they have their origin in our atmosphere or in that of the sun, it would be very desirable, in the first place, to make the experiments on the solar heat-spectrum shortly before sunset—when, it is well known, the atmospheric lines of the luminous part of the spectrum come out more distinct,—and also on high mountain-ridges, where the disturbing influence of the humid atmosphere of our regions is almost entirely excluded.

From the above-mentioned parallel experiments with rock-salt apparatus it clearly follows that the ultra-red rays of the atmosphere are strongly absorbed. In this absorption of the ultra-red rays lies the reason that the maximum of thermal effect in the solar spectrum changes its position with increasing altitude of the sun. In all the experiments which took place between 7 and 10 A.M. the maximum was after the first break, and was almost exactly as far from the line D as this latter from the line F. On the contrary, in many of the experiments made towards noon the thermal effect after the last break was quite as great as that maximum, or even exceeded it; and this was observed with peculiar distinctness in an experiment which took place on a cool day in October. It must therefore be admitted that, properly, here (after the last break) is the place where in the solar spectrum the thermal effect first attains its maximum.

This, in the rock-salt spectrum, is just as far from the line D as the latter from G.

Of the four different maxima of thermal effect observed by us in the solar spectrum, in most of our experiments the first alone kept its position, while the other three moved towards the red as the sun's altitude increased; consequently the first break appeared narrower in the experiments towards noon than in the morning ones.

It is very probable that the displacement of these maxima resulted from a change in the refractive power of the rock-salt prism, which, in the experiments towards noon, may have taken place through a strong heating of the prism. In order to test this supposition, I determined the minimum deflection for the line D at apartment-temperatures of  $5^{\circ}$  and  $16^{\circ}$  C.; and I found that, for a rock-salt prism of  $60^{\circ} 18'$  refracting angle, the deflection was about  $2'$  greater at  $16^{\circ}$  than at  $5^{\circ}$  C. The displacement of the maxima in my experiments is rather less than  $\frac{1}{2}$  millim., which corresponds to an angle that can be obtained by heating the rock-salt prism  $10^{\circ}$  C.; and such a heating could easily take place in my noon experiments, since the temperature of the room at that time was always very high.

In *all* the experiments (which were made with the three prisms above mentioned, at different hours of the forenoon, and at different seasons of the year) it was distinctly seen that the heat-effect of the solar spectrum, having attained its last maximum (after the last break), suddenly sinks. This was especially distinct in the experiments made about noon with rock-salt apparatus. I will further particularly mention that, in these experiments, neither the slit which was placed in the focus of the first lens, nor the double slit in front of the thermo-apparatus had more than  $\frac{1}{4}$  millim. breadth; further, a single shifting of the thermo-apparatus amounted to rather less than  $\frac{1}{4}$  millim.; and yet, under these circumstances, the deflections diminished one half at the place after the last maximum, when only two such shiftings had taken place. We are therefore entitled to ask, Is not the limit of refraction situated at the place where the heat-effect of the solar spectrum attains its last maximum? and does not the thermal effect observed beyond that maximum arise from diffused reflected heat?

In order to exclude the effect of diffused heat in the investigation of the individual parts of the spectrum, I made use of the method proposed by Geh. Rath Helmholtz, the one in which two prisms are employed, and by which he made the ultra-violet rays immediately visible to the eye by excluding the diffused light. I adopted this method several times in these investigations, especially in those cases in which it was necessary to separate the luminous from the obscure heat.

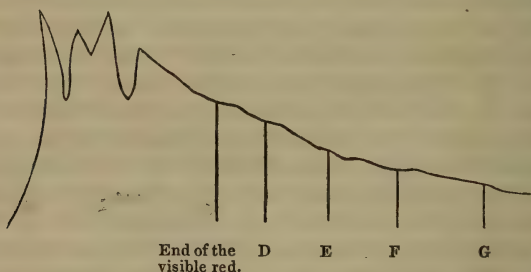


Unfortunately I was prevented by unfavourable weather from bringing to a conclusion the investigations, thus commenced, on the limit of refrangibility; so that I am not yet in a position to allege direct proofs in favour of the above-mentioned conjecture.

As regards the position of the maximum of thermal effect in the flint-glass spectrum, it also is found outside the red. In all the experiments made in June and July, I found it before the first break; in those made in October, on the contrary, it was after the same. In the different positions of the maximum of heat-effect in the solar spectrum may also lie the reason that the statements of the various investigators on the subject have been so different.

When in the solar spectrum we trace the heat-effect from the line D into the luminous portion, the deflections decrease gradually; with my arrangement I could perceive distinct thermal action beyond the line G. That this did not arise from diffused obscure heat was ascertained by means of the two-prism method above mentioned.

From my experiments I have projected the heat-curve for the solar spectrum, by taking the deflection for the maximum as 100, and reducing the others to this. All that has been said above can be readily seen in such a curve as the following, which represents the distribution of heat in the rock-salt spectrum.



If, in such a curve from the experiments with rock-salt apparatus, we compare the part corresponding to the obscure heat with that which corresponds to the luminous, we find that the former is twice as great; yet it is not strictly correct to reckon in this manner the ratio of the amount of the obscure to that of the luminous heat, because, as I shall further show, a certain amount of diffused obscure heat is spread over the luminous part of that spectrum.

## II. *Distribution of the Heat in the Spectrum of the Lime-Light.*

These experiments were carried out in the same manner as those on the heat-spectrum of the sun. Between the incandescent lime cylinder and the slit a lens of short focal distance

was placed, and at twice that distance from each of them. The distribution of the heat in the spectrum of the lime-light was likewise investigated both with flint-glass prisms and with rock-salt apparatus.

In the experiments with flint-glass prisms I was obliged to work with a rather broad slit (2 millims.), as the thermal effects were very feebly expressed. In the luminous part of such a spectrum, I could only verify a slight heat-effect in the red and orange. From the red outwards to the ultra-red end the deflections gradually augmented until they attained a certain maximum; then a gradual diminution commenced, yet without that interruption of continuity which we have always seen in the solar spectrum. I will here mention that Tyndall, in the account of his well-known experiments on "Calorescence" (Phil. Trans. 1866), called attention to the circumstance that the discontinuous distribution of heat, first observed by Sir John Herschel in the solar spectrum, does not exist in the spectrum of artificial sources of light.

On comparing the distribution of heat in the spectrum of the lime-light with that in the solar spectrum, we find that the position of the maximum of heat-effect is much further from the end of the visible red in the former than in the latter. In other words, with the feebler sources of heat the intensity of heat-effect attained its maximum for rays of greater wave-length than with more powerful sources of heat. Moreover this result was to be expected; for we have here the same case as, for example, in the incandescence of a platinum wire—namely, that the higher the temperature of the incandescence ascends, the more rays of less wave-length are emitted by the wire.

Besides, also in the flint-glass spectrum of the lime-light we do not observe the sudden diminution of heat-effect which we have seen in the solar spectrum; this, I believe, was merely in consequence of the slit being broad in our experiments with artificial sources of light. But in the experiments with rock-salt apparatus, where the slit was not so broad as in those with flint-glass prisms, though broader than in the corresponding experiments on the heat-spectrum of the sun, after the maximum a place can always be pointed out where a very sudden diminution is perceptible. However, as was remarked above, the question as to the existence of an abrupt diminution, or the probable limit of refrangibility, can only be decided by the two-prism method and the employment of very fine slits.

We have already mentioned that, in the flint-glass spectrum of the lime-light, only feeble heat-effects could be ascertained in the red and orange; but when the same lime-light was analyzed with a rock-salt prism, and the heat-effect of its spectrum inves-

tigated, some could be perceived even in the blue, although this spectrum was no brighter than that obtained with the flint-glass prism. Moreover absorption-experiments with a plate of flint glass and with water gave a very distinct apparent absorption of the luminous heat. This result (which, if correct, shows that luminous heat is not absorbed by transparent bodies in the same proportion as light) induced me to investigate very carefully the absorption of luminous heat by flint glass, using for the purpose the above-mentioned method with two rock-salt prisms. Having in this way separated certain homogeneous rays of sunlight, I compared their heat-effect before and after the insertion of a plane-parallel plate of flint glass so arranged that the rays fell on it perpendicularly.

Such experiments were made for all the colours of the solar spectrum; and the absorption of heat amounted to:—

for red	. . .	12	per cent.
„ orange	. . .	10	„
„ yellow	. . .	7	„
„ green	. . .	6	„
„ blue	. . .	5	„

From this amount of absorbed heat it is necessary to deduct the perpendicularly reflected heat, which, for a flint-glass plate, is equal to 5 per cent. of the total incident heat.

It still remains a desideratum to try photometrically whether the coloured light is not absorbed in the same proportion; otherwise we must suppose that in our experiments diffused heat was not entirely excluded.

According to these experiments, then, it must be admitted that the strong heat-effect observed in the luminous part of the lime-light spectrum in the above-mentioned experiments, arose chiefly from heat-rays which, in the rock-salt prisms, as in turbid media, were deflected by diffuse reflection.

Experiments were also made on the absorption of the ultra-red rays by transparent bodies, such as water, glass, mica, quartz, and calc-spar. From the end of the visible red to the place in the ultra-red part where thermal action quite ceased or at least was very feeble, the heat-effect was compared before and after the insertion of the bodies mentioned. It now appeared that, as Melloni (*Pogg. Ann.* 1832) had previously found with respect to water, *the ultra-red rays in passing through transparent bodies suffer a greater loss the less their refrangibility*. In regard to the displacement of the maximum in the spectrum of the lime-light after the insertion of the above-mentioned bodies, it depends on the thickness of the body inserted. For example, a layer of water of 2 millims. thickness caused no displacement of the



maximum; while a distinct displacement occurred with a layer of 10 millims. thickness.

Permit me to add one remark, that, from all the experiments I have made on the ultra-red rays of the solar spectrum in different hours of the forenoon, I hold myself justified in the assumption that the absorptive part of the atmosphere follows the same law; for I have always seen that the intensity of the rays of less refrangibility was always less in the morning, when the solar rays had to travel a much longer path through our humid atmosphere, than towards noon.

#### XXXIV. On the Theory of the Aberration of Light.

By Professor CHALLIS, M.A., LL.D., F.R.S.\*

SINCE the publication of my "Note on the Aberration of Light" in the Number of the Philosophical Magazine for June 1855, several experiments have been made for the purpose of settling a question which was incidentally adverted to in that communication—namely, whether or not any effect is produced on the amount of aberration by the transmission of the light through the lenses of the telescope. The experiments I refer to are:—those of M. Klinkerfues, described in a pamphlet entitled *Die Aberration der Fixsterne nach der Wellentheorie*, which was published at Leipsic in 1867; those of M. Hoek, the details of which are given in No. 1741 of the *Astronomische Nachrichten*; and the experiments made last year at the Greenwich Observatory, the results of which are stated by the Astronomer Royal in No. 130 of the 'Proceedings of the Royal Society' (p. 35), where also reference is made to the discussion of the question by the two experimenters just named. Each of these sets of experiments was made with a telescope the tube of which was filled through a certain length with a fluid. M. Klinkerfues, who used oil of turpentine, pointed his telescope to the sun and to stars, and supposed that his observations gave considerable aberration due to the fluid. M. Hoek contested this inference in Nos. 1669 and 1741 of the *Astronomische Nachrichten*, and showed that with a telescope partly filled with water and directed to a terrestrial object no aberration could be detected. This result, however, did not conclusively prove that no aberration due to the water would have been obtained if the telescope had been directed to a star. The Greenwich observations were made entirely on  $\gamma$  Draconis, and gave, on being reduced, an amount of aberration not sensibly differing from that in the 'Nautical Almanac,' and thus showed that very little or no aberration was

\* Communicated by the Author.

produced by the water. Having stated these experimental results, I proceed to discuss generally the Theory of Aberration, with reference more especially to the effect of a water-column in the tube of the telescope.

It will be proper first to define particularly the optical centre and axis of an optical instrument. In the case of a reflector having a circular object-mirror, the surface of which is spherical, the axis is perpendicular to the mirror at its middle point; and this point is the optical centre of the instrument, because the axes of all central beams from points of the object pass through it, as do also the axes of all the corresponding reflected beams. In a refractor, such as an astronomical or a Galilean telescope, the optical centre of the lens on which the central beams from the points of the object are *immediately* incident is the optical centre of the instrument, because that point is unique in the respect that the straight line joining the foci of each set of incident and refracted central beams passes, *quam proxime*, through it. No such point belongs to the second glass of a compound object-glass, nor to the combination of the two, if they have different refractive powers. This optical centre, or its position, will be designated by the letter O. The *axis* of a refractor cuts the surfaces of all the lenses and mirrors at right angles.

Next it is to be observed that any aberration that may be due to water in the telescope-tube is not produced by the change of *direction* which the rays undergo by ordinary refraction through the water, because the column, supposed to be symmetrical with respect to the axis of the instrument, only acts like an additional lens, altering the focal length of the telescope by changing the direction of the rays, just as the second lens of the object-glass alters it by being combined with the first. There is no more reason to attribute any aberrational effect to the refraction produced by the water than to that produced by the second lens, or any other lens of the instrument. The change of focal length has no effect on the amount of aberration, for the reason that aberration is sensibly the same in a long telescope as in a short one.

In refracting telescopes the axis of a pencil of rays which proceeds from an external point P and eventually forms an image in the field of view, may generally be assumed to be coincident, before incidence on the object-lens, with the axis of the central beam of which the pencil is a part, and consequently to pass through O, the optical centre above defined. After transmission through the *first* glass, the pencil would form, if its course were not changed by the second glass, an image at a point Q on the line P O produced. In the case of a telescope such as the Galilean, the axis of the original pencil from P is not coincident with

the axis of the beam incident on the object-lens, and consequently does not pass through  $O$ , the optical centre. Yet, as is known from optics, if it were not diverted from its course by the second lens, it would form an image at some point  $Q$  in  $PO$  produced. Supposing, therefore,  $OA$  to represent the direction of the pointing of the axis in the case of any refracting telescope, according to optics the course of the ray from the point  $P$  to the point where the image of  $P$  is formed in the field of view is made up of rectilinear parts determined as to position by the lenses or mirrors of the instrument, and inclined to the axis by angles having to the angle  $POA$  certain constant ratios independent of the particular position of  $P$ . Now, from what has been argued respecting the water-column, this theorem is as applicable to the part of the course which lies within the water as to the other parts, and is therefore true with respect to the inclinations of those rays to the axis which finally form the image in the field of view of the telescope.

When fixed or movable micrometer-wires or graduated scales are placed in the field of view so as to be distinctly visible together with the image of the object for the purpose of taking measures, the usual mode of determining the value in arc of the micrometer-revolution, or scale-interval, by transits of a star (as was done in the Greenwich experiments), gives the means of converting the final inclinations of the rays to the axis into the inclinations  $POA$  corresponding to celestial arcs, and virtually transfers the image in the field of view to a point  $Q'$  in  $PO$  produced, the distance of which from  $O$  is very nearly the same as that of the actual image. As far as aberration is concerned, it makes no difference whether we take the actual position of the image or the virtual position in  $PO$  produced. For the sake of simplicity I shall always suppose the image to be in the virtual position.

It has been supposed that the direction of a ray refracted by the object-glass of a telescope may be in some degree altered by the motion which the glass has relative to the course of the incident ray in consequence of the earth's orbital motion. But since the refraction at any small portion of a curved surface takes place ultimately as if that portion coincided with a tangent-plane, the curvature only determining the degree of convergency of the refracted pencil, it follows that the *direction* of the refracted ray is not altered by the motion of the surface, even if there be physical reasons for concluding that the change of direction from that of incidence to that of refraction is not instantaneous, but occupies a very small interval of time, provided always that there is no sensible *angular* motion of the refracting surface. Hence also the motion of the object-lens produces no



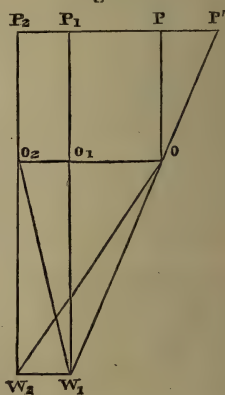
perceptible change in the position of its optical centre, that position being determined by refractions at surfaces which may be regarded as plane and parallel. Consequently the motion of the telescope has no aberrational effect. In fact, the experiment of M. Hoek gave no indication of aberration from this cause, although it was well adapted for detecting it, inasmuch as the sign of such aberration would have been different according as the telescope was directed northward or southward.

What has been said in the four preceding paragraphs with respect to a refracting telescope is applicable, *mutatis mutandis*, to a reflector.

From the foregoing general considerations I proceed to the theory of aberration, as it respects especially the experiments made with water in the tube of the telescope. In speaking of the motion of the telescope or of its optical centre, it is to be understood that the actual motion resolved perpendicularly to the direction of the pointing of the telescope, which is the same as the product of the earth's motion and the sine of the "earth's way," is alone taken into account, the part resolved along the direction of vision being ineffective as regards aberration.

Taking, first, the case of M. Hoek's experiment, in which the telescope was directed in or near the meridian, both northward and southward, about noon or midnight, to an object which could be made to revolve about a vertical axis with the telescope, let O and P (fig. 1) be the positions of the optical centre and the object at a given instant, and let the straight line joining O and P be a prolongation of the axis of the telescope. Then the ray which at the given instant arrives at O must have left the object when it had a position P' such that if the earth's motion be in the direction from P' to P, the line P'P is to P'O as the velocity of the object to the velocity of light. Although the direction P'O of the incident ray is inclined to the axis-direction OP, according to the antecedent argument the ray forms an image which may be assumed to be at some point W<sub>1</sub> in P'O produced. Leaving at first out of account any retardations of the light resulting from the transmissions through the water and the lenses, while the light is propagated from O to W<sub>1</sub> let the object move from P to a point P<sub>1</sub> in P'P produced, and let the optical centre move through an equal parallel space OO<sub>1</sub>. We shall thus have the ratio of W<sub>1</sub>O to OO<sub>1</sub>, the same as that

Fig. 1.

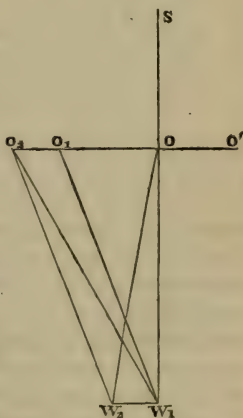


of  $OP'$  to  $P'P$ ; so that  $W_1O_1$ , which is necessarily the *instrumental* direction of the object at the instant the image is seen at  $W_1$ , is parallel to  $OP$  and points to  $P_1$ , the position of the object at that instant. Hence there is no aberration.

Supposing, now, the ray to be retarded by passing through the lenses and the water, the argument above referred to shows that the image of the object may still be assumed to be at some point  $W_1$  in  $P'O$  produced, although the distance  $OW_1$  will not be the same as before. Draw  $P_1O_1W_1$  parallel to  $PO$ . Then during the passage of the light from  $O$  to  $W_1$  the object moves through a space  $PP_2$  greater than  $PP_1$ , and the optical centre through a space  $O O_2$  equally greater than  $OO_1$ . Thus the instrumental line of collimation is  $W_1O_2$ , which does not point to  $P_2$ ; and consequently, if  $P_2O_2W_2$  be drawn parallel to  $P_1O_1W_1$ , there is a forward aberration equal to the angle  $W_1O_2W_2$ . But M. Hoek found no aberration, the reading of the micrometer for bisection of the object being, *quam proxime*, the same whether the telescope was directed northward or southward; whereas if such aberration existed, it would have had opposite signs in the two positions of the telescope. To account for this fact it is necessary to admit that the ray, after passing through  $O$ , is *dragged* by the water and the lenses, so that the image is formed at a point  $W_2$  such that  $W_1W_2 = O_1O_2 = P_1P_2$ . The instrumental direction of vision thus becomes  $W_2O_2$  pointing to the object  $P_2$ ; and hence there is no aberration. I shall presently endeavour to give a theory of this dragging of the ray.

In the case of the Greenwich experiment, in which the telescope was directed to a star, let  $O$  (fig. 2) be the position of the optical centre at any instant, and let  $OS$ , pointing to the star, be in the direction of the prolongation of the axis. Then the pencil of rays which, starting from  $S$ , arrives at  $O$  at the same instant that the optical centre, moving in the direction from  $O'$  to  $O$ , arrives at the same point, proceeds to form an image at some point  $W_1$  in  $SO$  produced. In the case of no retardation by the lenses and the water, let the optical centre move from  $O$  to  $O_1$ , while the light by which the image is seen at  $W_1$  is propagated from  $O$  to  $W_1$ . Then, since  $W_1O$  is the instrumental direction of the star at the instant of the bisection of its image at  $W_1$ , the aberration is the angle  $OW_1O_1$ , the value of which is that given by the usual formula of correction for aberration.

Fig. 2.



Supposing the ray to suffer retardation by passing through the lenses and through a column of water in the telescope-tube, the image of S may still be assumed to be at some point  $W_1$  in SO produced, although at a different distance from O. Draw  $W_1 O_1$  making the same angle with  $OW_1$  as in the case of no retardation. Then, by reason of the retardation, the optical centre moves through a space  $OO_2$  greater than  $OO_1$ , and thus there would appear to be the additional aberration  $O_1 W_1 O_2$ . But M. Hoek's experiment proves that this amount of aberration is exactly neutralized by the dragging of the ray, the effect of which is to shift the position of the image from  $W_1$  to a point  $W_2$  such that  $W_1 W_2$  is parallel and equal to  $O_1 O_2$ . Accordingly the instrumental direction of the star is  $W_2 O_2$ , which is parallel to  $W_1 O_1$ ; so that the aberration is the same as if the lenses and the water had no effect. This agrees with the result of the Greenwich experiment.

There remains the question, What is the cause of the dragging or displacement of the ray? and why is the amount of displacement just *equal* to the increment of the aberration due to the lenses and the water?

According to the view I take of the Undulatory Theory of Light, this question appears to admit of the following answer. I have constantly maintained that the propagation of light is caused to be slower in a medium than in vacuum simply by the obstacle to the free movement of the æther due to the reflex action of the finite atoms of the medium, the effect of this action at any given point being the result of reflections, unaccompanied by sensible condensation, from a vast number of neighbouring atoms. Thus although the actual elasticity of the æther may be the same within the medium as outside, there is by this action an *apparent* diminution of elasticity, in consequence of which the relation  $v = a'\sigma$  between the velocity  $v$  and condensation  $\sigma$  of the fluid out of the medium is changed to  $v = \frac{a'}{\mu}\sigma$  within,  $\mu$  being the refractive index of the medium and  $a'$  the rate of propagation of light in vacuum. But this being the case when the æther is in motion and the medium at rest, there must be a corresponding action when the medium is in motion and the fluid at rest. The two actions are not, however, immediately comparable one with the other—because, with respect to movements communicated to the æther by the atoms of the medium, there is no parameter of the same order as  $\lambda$  in light-undulations, on the value of which the amount by which the propagation is retarded within the medium depends, being greater as  $\lambda$  is less. We may, however, regard the impulsive action of the atoms as an extraneous impressed force, which, if measured in the same manner as the elastic force of the æther, will be proportional,



*cæteris paribus*, to the square of the earth's velocity, because the motion of the æther which it generates is propagated in space with that velocity, inasmuch as it travels with the earth. But besides this, such impressed force evidently vanishes if  $\mu=1$ . Therefore, putting  $V$  for the earth's velocity, let us assume that this force acting in the direction  $OO_1$  (figs. 1 and 2) is measured

by  $V^2 \left(1 - \frac{1}{\mu}\right)^2$ . Then on the same scale the elastic force in

$OW_1$  is measured by  $\frac{a'^2}{\mu^2}$ . The former force would give rise to

a velocity of propagation equal to  $V \left(1 - \frac{1}{\mu}\right)$  in the direction

$OO_1$ , and the latter to a velocity of propagation  $\frac{a'}{\mu}$  in the direc-

tion  $OW_1$ . Consequently, if the angle  $W_1OW_2$  be equal to the

ratio of  $V \left(1 - \frac{1}{\mu}\right)$  to  $\frac{a'}{\mu}$ , or  $\frac{V}{a'} (\mu-1)$ , the resulting direction

of propagation will be in  $OW_2$ . This in fact is the value of that angle which, as already shown, satisfies the phenomena of aberration. Hence the dragging of the ray assumed in the theory of aberration is in this manner accounted for on the Undulatory Theory of Light.

Independently of the truth of the foregoing theoretical explanation, it may safely be asserted to be a consequence of the antecedent argument respecting aberration, that whatever accounts for the displacement of the ray in M. Hoek's experiment accounts for it also in the Greenwich experiment, and that the fact that no aberration beyond the usual amount was obtained by the latter necessarily results from the non-existence of aberration established by the other.

Cambridge, March 16, 1872.

XXXV. *An Attempt to Account for Anomalous Dispersion of Light*. By OSCAR EMIL MEYER\*.

IN this memoir I communicate a theory which I projected in the year 1863, to account for the anomalous dispersion which was then known only in the case of the metals and the vapour of iodine. I have hitherto refrained from publishing it, because it did not appear to me quite satisfactory. But I think that I ought no longer to withhold it, now that the discovery of

\* Translated from a separate copy, communicated by the Author, from Poggendorff's *Annalen*, vol. cxlv. pp. 80-86.

anomalous dispersion in a very great number of substances has turned general attention to the subject.

Anomalous dispersion of light appears to occur only in those bodies which very strongly absorb light, so as to be almost opaque. This coincidence is the more striking, as at the same time other optical properties are almost always found in combination with these two: namely, the bodies exhibit likewise elliptic polarization of the reflected light, and frequently a surface-colour different from the body-colour.

That elliptic polarization and anomalous dispersion are combined in the case of metals was observed by Brewster\*. In Cauchy's theory of metallic reflection† elliptic polarization appears to be necessarily united with strong absorption, *i. e.* opacity; this we learn from the structure of his formulæ, in which, to the periodic functions representing the oscillations, exponential functions are added which measure the weakening of the light. It is moreover well known that several metals, and gold above all, transmit light of a different colour from that which they reflect—that is, possess a body-colour different from the colour of their surface.

The same number of properties are found united in other classes of bodies besides the metals—namely, in the many strongly coloured substances whose anomalous dispersion has been observed by Leroux‡, Christiansen§, Kundt||, and Soret¶. Among these bodies, Carthamin must above all be mentioned as that substance of which all those properties have been clearly demonstrated. Stokes in 1853\*\* referred to the close connexion, in these bodies, between elliptic polarization of reflected light, their different body- and surface-colours, and their strong absorptive power. Since Kundt has now recently indicated, in his first memoir, the connexion of anomalous dispersion with the surface-colour, and, in his second and third, with the absorption of light, it can hardly be doubted that all these properties occur simultaneously in non-metallic bodies also.

It is natural to regard as causal the most striking of these properties, that which shows itself most distinctly with *all*—that is, opacity. This will be most simply explained by the hypothesis of a resistance undergone by the oscillating particles of

\* Phil. Trans. 1830, p. 325. Pogg. Ann. vol. xxi. (1831).

† *Comptes Rendus*, vol. viii. p. 553 (1839). Compare Beer, Pogg. Ann. vol. xci. p. 561 (1854); and F. Eisenlohr, Pogg. Ann. vol. civ. p. 368, (1858).

‡ *Comptes Rendus*, vol. lv. p. 126. Phil. Mag. S. 4. vol. xxiv. p. 245.

§ Pogg. Ann. vol. cxliii. p. 250 (1871). Phil. Mag. S. 4. vol. xli. p. 244.

|| Pogg. Ann. vol. cxl. p. 163; vol. cxliii. p. 259; vol. cxliv. p. 128 (1871).

¶ *Arch. des Sc. Phys.* March 1871. Pogg. Ann. vol. cxliii. p. 325.

\*\* Phil. Mag. S. vol. vi. p. 293. Pogg. Ann. vol. xci. p. 300.

æther in such media. This resistance must depend on the velocity of the particles, and vanish with them. On the supposition of small amplitude and consequently little velocity of the oscillations, the assumption that these resisting forces are proportional to the velocities appears perfectly safe.

A so much the more weighty doubt, however, must arise as to whether the seat of the assumed force is in the ponderable matter, or whether the force upon an oscillating æther-particle is exerted by the neighbouring particles of æther. In the first case we may without hesitation regard the ponderable particles as motionless, and accordingly we have to suppose this force proportional to the absolute velocity of the æther-particle on which it operates. In the other case, on the contrary, considering the force a reciprocal operation of the oscillating particles of æther, we must suppose its value proportional to their *relative* velocities. The latter hypothesis thus leads to the assumption of an *internal friction* in the luminous æther of semitransparent media, while the former hypothesis introduces a force comparable to what is called the *external* friction of liquids.

As is well known, the differential equation for a plane undulatory motion is

$$\frac{d^2v}{dt^2} + \mu^2 \frac{d^2v}{dx^2}.$$

In this,  $v$  denotes the displacement which exists at the time  $t$ , in a plane at the distance  $x$  from the origin of the undulations;  $\mu$  is a constant. The equation is solved by the formula

$$v = A \cos(\alpha t - \beta x) + B \sin(\alpha t - \beta x),$$

in which  $A$ ,  $B$ ,  $\alpha$ ,  $\beta$  are constant quantities. The two latter are so related that

$$\alpha = \mu \beta;$$

and this relation teaches that  $\mu$  is the velocity of propagation of the wave. Taking as unit the velocity *in vacuo*, the index of refraction

$$n = \frac{\beta}{\alpha} = \frac{1}{\mu}.$$

After the new hypothesis, the above differential equation has to be completed by an additional term, which is either

$$-\kappa \frac{dv}{dt} \text{ or } \nu \frac{d^3v}{dt dx^2},$$

according to whether the one or the other hypothesis be adopted.  $\kappa$  and  $\nu$  are constant quantities of positive value; the latter was named by Stokes\* the index of friction; the former might ana-

\* Camb. Phil. Soc. Tr. vol. ix. pt. 2. p. 17.



logously be called the index of the external friction of the æther on the ponderable molecules.

According to the first of the two hypotheses, we have therefore to put

$$\frac{d^2v}{dt^2} = u^2 \frac{d^2v}{dx^2} - \kappa \frac{dv}{dt}.$$

This equation is satisfied by the function

$$v = [A \cos(\alpha t - \beta x) + B \sin(\alpha t - \beta x)] e^{-\gamma x}$$

when between the constants  $\alpha$ ,  $\beta$ ,  $\gamma$  the relations

$$\alpha^2 = \mu^2(\beta^2 - \gamma^2),$$

$$\kappa \alpha = 2\mu^2 \beta \gamma,$$

subsist. Hence we obtain

$$2\mu^2 \beta^2 = \alpha^2 (1 + \sqrt{1 + \kappa^2 \alpha^{-2}}),$$

and further, for the determination of the quotient of refraction  $n$ , taking as unit the velocity of light in empty space,

$$n^2 = \frac{\beta^2}{\alpha^2} = \frac{1}{2\mu^2} (1 + \sqrt{1 + \kappa^2 \alpha^{-2}}).$$

Putting now

$$\alpha T = 2\pi,$$

$T$  is the time of an undulation of the ray; this, however, with the chosen unit of velocity, is equal to the wave-length  $\lambda$  in *vacuo*,

$$T = \lambda,$$

so that we may also put

$$\alpha \lambda = 2\pi.$$

We have therefore, according to this hypothesis,

$$n^2 = \frac{1}{2\mu^2} \left\{ 1 + \sqrt{1 + \left( \frac{\kappa \lambda}{2\alpha} \right)^2} \right\}.$$

According to this formula, with increasing value of the wave-length  $\lambda$  the ratio of refraction likewise becomes greater. This law is exactly the opposite of the ordinary law of dispersion, and therefore comprehends anomalous dispersion.

To an altogether similar result we are conducted by the other theory, which assumes internal friction of the æther. The differential equation here is

$$\frac{d^2v}{dt^2} = \mu^2 \frac{d^2v}{dx^2} + \nu \frac{d^2v}{dt dx^2}.$$

This equation also is integrated by the function

$$v = [A \cos (\alpha t - \beta x) + B \sin (\alpha t - \beta x)] e^{-\gamma x};$$

but  $\alpha$ ,  $\beta$ ,  $\gamma$  have to satisfy the equations

$$\alpha^2 = \mu^2 (\beta^2 - \gamma^2) + 2\nu\alpha\beta\gamma,$$

$$0 = 2\mu^2\beta\gamma - \nu\alpha(\beta^2 - \gamma^2).$$

Hence we obtain, for the quotient of refraction  $n$ ,  $\frac{n}{\alpha}$

$$n^2 = \frac{\beta^2}{\alpha^2} = \frac{1}{2} \frac{\mu^2}{\mu^4 + \nu^2 \alpha^2} + \frac{1}{2} \frac{1}{\sqrt{\mu^4 + \nu^2 \alpha^2}};$$

so that according to this theory also, if we again put

$$a\lambda = 2\pi,$$

there results for the quotient of refraction  $n$  an expression,

$$n^2 = \frac{1}{2} \frac{\mu^2}{\mu^4 + \left(\frac{2\pi\nu}{\lambda}\right)^2} + \frac{1}{2} \frac{1}{\sqrt{\mu^4 + \left(\frac{2\pi\nu}{\lambda}\right)^2}}$$

which with increasing wave-length  $\lambda$  becomes itself of higher value.

Accordingly both theories explain anomalous dispersion of light, and both, too, from hypotheses which find their justification in the opacity of the medium. At the same time elliptic polarization of light results from both theories; for the formula

$$v = [A \cos (\alpha t - \beta x) + B \sin (\alpha t - \beta x)] e^{-\gamma x}$$

is the very one on which Cauchy grounded his theory of metallic reflection.

Our theories, however, fail us in one important point. It results from the values of  $\gamma$  in both cases that the light of shorter wave-lengths is more strongly absorbed than that of greater wave-lengths. Those bodies should therefore all appear red by transmitted light.

Further, on numerical comparison of the above formulæ with the observations of Jamin\* on metals, and those of Christiansen on Fuchsine, we find no satisfactory agreement.

The preceding theory, therefore, does not yet completely exhaust the subject. But since it gives an account of the most important circumstances, I hold it to be correctly founded, and I hope to be able to carry it further as soon as observation shall have given us a more perfect knowledge of the laws.

Breslau, Christmas 1871.

\* *Ann. de Ch. et de Ph.* 3rd series, vol. xix.; *Pogg. Ann.* Erg. Band ii.

XXXVI. *On Hyperdistributives.* By SIR JAMES COCKLE, F.R.S.,  
Corresponding Member of the Literary and Philosophical Society  
of Manchester, President of the Queensland Philosophical  
Society, &c.\*

1. LET  $u$  and  $a$  in

$$\theta(u) + \theta(a) = \theta(u+a) \quad . \quad . \quad . \quad . \quad . \quad (1)$$

be regarded only as recipients of suffixes, so that  $\theta(u)$  is a function, not of  $u$ , but of independent symbols  $u_0, u_1, u_2, \dots$ , and

$$\theta(u) = \theta(u_0, u_1, u_2, \dots) \quad . \quad . \quad . \quad . \quad . \quad (2)$$

2. Put

$$\theta(u+a) = \theta(A_0, A_1, A_2, \dots) \quad . \quad . \quad . \quad (3)$$

wherein

$$A_r = (u+a)_r \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Then, when we can interpret  $A_r$  so as to obtain uniform results, the function  $\theta$  may be called a hyperdistributive.

3. If

$$A_m = u_m + mu_{m-1}a_1 + \dots + a_m, \quad . \quad . \quad . \quad (5)$$

where the dexter is obtained from the development of  $(u+a)^m$  by changing exponents into suffixes, uniform results may be obtained.

4. Let, generally,

$$\theta_1(u) = u_1, \quad . \quad . \quad . \quad . \quad . \quad (6)$$

$$\theta_2(u) = u_2 - u_1^2, \quad . \quad . \quad . \quad . \quad . \quad (7)$$

$$\theta_3(u) = u_3 - 3u_1u_2 + 2u_1^3, \quad . \quad . \quad . \quad . \quad . \quad (8)$$

$$\theta_4(u) = u_4 - 4u_1u_3 - 3u_2^2 + 12u_1^2u_2 - 6u_1^4; \quad . \quad . \quad (9)$$

then we shall have

$$\theta_1(u) + \theta_1(a) = \theta_1(A), \quad . \quad . \quad . \quad . \quad (10)$$

$$\theta_2(u) + \theta_2(a) = \theta_2(A), \quad . \quad . \quad . \quad . \quad (11)$$

$$\theta_3(u) + \theta_3(a) = \theta_3(A), \quad . \quad . \quad . \quad . \quad (12)$$

$$\theta_4(u) + \theta_4(a) = \theta_4(A), \quad . \quad . \quad . \quad . \quad (13)$$

provided that, in the development of  $\theta(A)$ , we replace  $A_r$  by  $(u+a)_r$  and then change exponents into suffixes.

5. These results are all comprised in

$$\theta_m(u) + \theta_m(a) = \theta_m(A). \quad . \quad . \quad . \quad (14)$$

When  $m=2$ , or  $m=3$ , the results of the last article may be verified directly without any great labour. When  $m=4$  the calculation is rather longer; but much of the work is already done

\* Communicated by the Rev. Robert Harley, F.R.S.



in my paper "On Criticoids" in the Number of this Journal for March 1870. If we put

$$\psi(u) = u_4 - 4u_1u_3 + 3u_2^2, \quad \dots \quad (15)$$

then

$$\psi(A) = \psi(a) + 12\theta_2(a)\theta_2(u) + \psi(u), \quad (16)$$

whence, subtracting six times the square of (11),

$$\psi(A) - 6\{\theta_2(A)\}^2 = \psi(a) - 6\{\theta_2(a)\}^2 + \psi(u) - 6\{\theta_2(u)\}^2, \quad (17)$$

which reduces to (13).

6. Marking transitions, analogous to that from exponents to suffixes, by special brackets  $\{.\}$ ,  $[.]$ ,  $\{.\}$ , &c., I reserve the parenthesis  $(.)$  for ordinary involution. The common expansion of  $(u+a)^m$  may be written in either of the forms

$$(u+a)^m = (u)^m + m(u)^{m-1}(a)^1 + \dots + (a)^m, \quad \dots \quad (18)$$

$$(u+a)^m = (a)^0(u)^m + m(a)^1(u)^{m-1} + \dots + (a)^m(u)^0. \quad (19)$$

7. Following up the latter form, I shall put

$$\{u+a\}^m = [a]^0\{u\}^m + m[a]^1\{u\}^{m-1} + \&c., \quad \dots \quad (20)$$

the convention being that the symbol contiguous to a bracket in the undeveloped form is affected by that bracket in the development. There are, of course, corresponding developments of  $[u+a]^m$ ,  $\{u+a\}^m$ ,  $[u+a]^m$ , and of  $\{u+a\}^m$ ,  $(u+a)^m$ , &c. The symbols  $\{u\}^r$  are arbitrary, or may have one meaning and be connected by one law, while  $[a]^r$ , and indeed  $\{a\}^r$  are also arbitrary, and may have the same, or another meaning; and be connected by the same or another law.

8. Let it be a property of the square bracket  $[.]$  that

$$[u]^m = u_m, \quad \dots \quad (21)$$

and that

$$[u]^0 = u_0 = 1, \quad \dots \quad (22)$$

in the same way that for the parenthesis  $(y)^0 = y_0 = 1$ . Then

$$\{u+a\}^m = \{u\}^m + ma_1\{u\}^{m-1} + \dots + a_m\{u\}^0, \quad \dots \quad (23)$$

and (4) becomes

$$A_r = [u+a]^r. \quad \dots \quad (24)$$

9. We can now obtain general forms of hyperdistributives. An inspection of the identity

$$\frac{d^m}{dx^m} \left( \frac{1}{u} \frac{du}{dx} \right) + \frac{d^m}{dx^m} \left( \frac{1}{a} \frac{da}{dx} \right) = \frac{d^m}{dx^m} \left( \frac{1}{ua} \frac{d(ua)}{dx} \right) \quad \dots \quad (25)$$

shows that, in the development of the dexter,  $u$  and its differen-

tial coefficients will, after all reductions, be separated from  $a$  and its differential coefficients. Now, when in the development of

$\frac{d^m}{dx^m} \left( \frac{1}{u} \frac{du}{dx} \right)$  there occurs an expression  $\left( \frac{1}{u} \frac{d^n u}{dx^n} \right)^r$ , then there occurs

in the dexter of (25) the corresponding expression  $\left( \frac{1}{ua} \frac{d^n(ua)}{dx^n} \right)^r$ .

But if

$$\frac{du}{dx} = \{u\}, \quad . . . . . (26)$$

then, by Leibnitz's theorem, as expressed in brackets,

$$\frac{1}{ua} \frac{d^n(ua)}{dx^n} = \frac{\{u+a\}^n}{ua} = \frac{\{u+a\}^a}{\{u\}^0 \{a\}^0} . . . (27)$$

Make

$$\frac{\{u\}^r}{\{u\}^0} = u_r, \quad \frac{\{a\}^r}{\{a\}^0} = a_r, \quad . . . . . (28)$$

and we have

$$\frac{\{u+a\}^n}{\{u\}^0 \{a\}^0} = [u+a]^n . . . . . (29)$$

10. We know that  $\frac{d^m}{dx^m} \left( \frac{1}{u} \frac{du}{dx} \right)$  can be developed in terms of  $u_1$ ,  $u_2$ , &c., and  $\frac{d^m}{dx^m} \left( \frac{1}{a} \frac{da}{dx} \right)$  in terms of  $a_1$ ,  $a_2$ , &c. The article preceding shows that the expression for  $\frac{d^m}{dx^m} \left( \frac{1}{ua} \frac{d(ua)}{dx} \right)$  can be obtained from either development by writing therein  $[u+a]^r$  in place of  $u_r$  or  $a_r$  respectively. This is the same thing as replacing  $u_r$  or  $a_r$  by  $A_r$ .

11. Let, then,

$$\frac{d^m \log u}{dx^m} = \theta(u_1, u_2, \dots, u_m) = \theta_m(u), \quad . . . (30)$$

and  $\theta_m(u)$  will be a hyperdistributive, and may, I think, be called the primary hyperdistributive of the  $m$ th order. The symbols  $u_1$ ,  $u_2$ ,  $\dots$ ,  $u_m$  are independent; for  $u$  is an arbitrary function of  $x$ . As observed by the great De Morgan (Camb. Trans. vol. ix. part 2), undefined dependence is independence.

12. We may denote the  $n$ th power of  $\{u\}^r$  by  $\{u\}^{r(n)}$ , where  $r$  is the algorithmic index, and  $n$  or  $(n)$  an ordinary exponent. This notation, however, will not be much needed in what follows; for we can replace algorithmic indices by suffixes.

13. We know that

$$\frac{du_m}{dx} = u_{m+1} - u_1 u_m. \quad . \quad . \quad . \quad . \quad (31)$$

And hence we can deduce

$$\frac{d}{dx} [u - u_1]^m = [u - u_1]^{m+1} - m[u - u_1]^2 [u - u_1]^{m-1}. \quad . \quad (32)$$

Let us put

$$[u - u_1]^m = U_m, \quad . \quad . \quad . \quad . \quad (33)$$

and (32) becomes

$$\frac{dU_m}{dx} = U_{m+1} - mU_2 U_{m-1}. \quad . \quad . \quad . \quad (34)$$

14. Recurring to (30), we see that

$$\theta_m(u) = \frac{d^{m-2}}{dx^{m-2}} \cdot \frac{d^2 \log u}{dx^2} = \frac{d^{m-2} U_2}{dx^{m-2}}; \quad . \quad . \quad . \quad (35)$$

and the dexter of this result, regarded in the light of (34), suggests that  $\theta_m(u)$  may be expressed in terms of  $U_2, U_3, \dots, U_m$ . The quantity  $U_1$  vanishes identically.

15. Up to the eighth order inclusive, I find the hyperdistributives as follows:—

$$\begin{aligned} \theta_2(u) &= U_2, \quad \theta_3(u) = U_3, \quad \theta_4(u) = U_4 - 3U_2^2, \\ \theta_5(u) &= U_5 - 10U_2 U_3, \quad \theta_6(u) = U_6 - 15U_2 U_4 - 10U_3^2 + 30U_2^3, \\ \theta_7(u) &= U_7 - 21U_2 U_5 - 35U_3 U_4 + 210U_2^2 U_3, \\ \theta_8(u) &= U_8 - 28U_2 U_6 - 56U_3 U_5 - 35U_4^2 + 70U_2 (6U_2 U_4 \\ &\quad + 8U_3^2 - 9U_2^3). \end{aligned}$$

16. If we denote by  $P_m$  the terms, or the aggregate of the terms, in  $\theta_m(u)$  which involve triple, or higher, partitions of  $m$ , all the foregoing results will be comprised in the formula

$$\theta_m(u) = 2U_m - \frac{1}{2}[U + U]^m + P_m. \quad . \quad . \quad . \quad (36)$$

17. By way of example,

$$\begin{aligned} U_4 &= [u - u_1]_4 = u_4 - 4u_1 u_3 + 6u_1^2 u_2 - 4u_1^3 u_1 + u_1^4, \\ 3U_2^2 &= 3[u - u_1]^{2(2)} = 3(u_2 - 2u_1 u_1 + u_1^2)^2 = 3(u_2 - u_1^2)^2. \end{aligned}$$

Hence, developing the last expression and subtracting,

$$U_4 - 3U_2^2 = u_4 - 4u_1 u_3 - 3u_2^2 + 12u_1^2 u_2 - 6u_1^4, \quad . \quad . \quad (37)$$

which agrees with (9).

18. All linear functions of hyperdistributives are hyperdistributives.



butive. Moreover if  $f_m(u)$  be any distributive function of any quantity  $u$ , and

$$\theta_m(u) + f_m(u) = \mathfrak{S}_m(u), \quad . \quad . \quad . \quad . \quad (38)$$

then  $\mathfrak{S}_m(u)$ , or any linear function of such functions, is hyperdistributive. The suffix-recipient  $u$  in  $\theta(u)$ , and the explicit quantity or variable  $u$  in  $f(u)$ , are independent, and have no connexion. But we may if we please establish a connexion between  $u_1, u_2, \dots$  &c., and between them and the  $u$  in  $f(u)$ .

19. Thus, let  $f(u)$  vanish, and suppose that, for all values of  $r$ ,

$$[u]^r = (u)^r = u^r = u_1^r. \quad . \quad . \quad . \quad . \quad (39)$$

Then

$$U_r = [u - u_1]^r = (u_1 - u_1)^r = 0. \quad . \quad . \quad (40)$$

Hence (14) will take the form

$$\theta_m(a) = \theta_m(A) \quad . \quad . \quad . \quad . \quad . \quad (41)$$

the meaning of which result is, that  $\theta_m(a)$  will remain unaltered when, in it, we replace  $a_r$  by  $[a + u]^r$ , or its equivalent  $(u + a)^r$ . When (39) is satisfied, (14), becoming (41), gives us the entire critical functions. It must be borne in mind that (39) does not imply any such relations as

$$[a]^r = (a)^r = a^r = a_1^r. \quad . \quad . \quad . \quad . \quad (42)$$

20. Again, let

$$f_m(u) = f_m(u_1) = -\frac{d^{m-1}u_1}{dx^{m-1}}, \quad . \quad . \quad . \quad . \quad (43)$$

and, for all values of  $r$ , let

$$u_r = \frac{1}{u} \frac{d^r u}{dx^r}. \quad . \quad . \quad . \quad . \quad . \quad (44)$$

Then

$$\theta_m(u_1, u_2, \dots, u_m) - \frac{d^{m-1}u_1}{dx^{m-1}} = 0, \quad . \quad . \quad . \quad (45)$$

whatever  $u$  may be, and the hyperdistributive relation

$$\begin{aligned} \theta_m(u_1, u_2, \dots) - \frac{d^{m-1}u_1}{dx^{m-1}} + \theta_m(a_1, a_2, \dots) - \frac{d^{m-1}a_1}{dx^{m-1}} \\ = \theta_m(A_1, A_2, \dots) - \frac{d^{m-1}A_1}{dx^{m-1}}. \quad . \quad . \quad . \quad . \quad (46) \end{aligned}$$

becomes

$$\theta_m(a_1, a_2, \dots, a_m) - \frac{d^{m-1}a_1}{dx^{m-1}} = \theta_m(A_1, A_2, \dots, A_m) - \frac{d^{m-1}A_1}{dx^{m-1}}; \quad . \quad (47)$$

and the meaning of this result is, that  $\theta_m(a_1, a_2, \dots, a_m) - \frac{d^{m-1}a_1}{dx^{m-1}}$

will remain unaltered when, having replaced  $a_r$  by  $[a+u]^r$ , we substitute for  $u_r$  its value as given by (44). If we assume  $u$  to be a rational and entire function of  $x$ , its degree should not be less than  $m$ , or our form will be needlessly restricted. We thus obtain the entire criticoidal functions. The symbols  $a$  and  $A$  are not in general subject to any relation corresponding to (44). I say entire critical and criticoidal functions, because there are other such functions, as will be seen on reference to my paper "On Fractional Criticoids" in the last May (1871) Number of this Journal\*.

21. All functions of critical or criticoidal functions are, respectively, critical or criticoidal. And critical functions and criticoids are thus seen to be not merely connected by the analogies of algebra and the calculus, but to have, in hyperdistributives, a common algorithmic origin.

"Oakwal" near Brisbane, Queensland,  
Australia, January 23, 1872.

### XXXVII. Notices respecting New Books.

*Monthly Notices of the Royal Astronomical Society. The President's Address on the Presentation of the Gold Medal, Feb. 1872.*

*Observations of Comets from B.C. 611 to A.D. 1640. Extracted from the Chinese Annals by JOHN WILLIAMS, F.S.A. London, 1871.*

**A**MIDST the eager pursuit of information capable of throwing additional light on the physics of the sun, it is refreshing to find the Royal Astronomical Society directing its renewed attention

\* Phil. Mag. S. 4. vol. xli. No. 274, pp. 360-368. Some of the foregoing formulæ involve questions of partition. The following notation of partitions seems to be convenient. Put

$$\sum_{r=1}^{r=n} \psi(r) = S_n \psi(r),$$

and let  $\pi(n)$  denote the number of different partitions of  $n$ , including  $n$  itself. Also let  $\pi_m(n)$  denote the number of different partitions of  $n$  into  $m$  parts, no zero part occurring. Let  $\phi(n)$  denote the number of different partitions of  $n$  in which no unit part occurs. Also let  $\phi_m(n)$  denote the number of different partitions of  $n$  into  $m$  parts, no unit or zero part occurring. Then

$$\pi(n) = S_n \pi_r(n); \quad \phi(n) = S_n \phi_r(n);$$

$$\phi_m(n+m) = \pi_m(n) = S_m \pi_r(n-m);$$

$$\pi(2n) = 1 + S_n \pi(r) + S_{n-1} \pi_r(2n);$$

$$\pi(2n+1) = 1 + S_n \{ \pi(r) + \pi_r(2n+1) \};$$

and consequently

$$\pi_m(2m) = \pi(m).$$

Thus

$$\pi(4) = 1 + \pi(2) + \pi(1) + \pi_1(4) = 5$$

exemplifies the notation.

*Phil. Mag. S. 4. Vol. 43. No. 236. April 1872.*

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in the award of its Gold Medal, to the remarkable connexion which subsists between comets and meteors; and it is gratifying to find that the recipient is an astronomer (Signor Schiaparelli) who has been successful in establishing this connexion. There is, however, something of a still higher character to which these researches lead us. The late President of the Royal Astronomical Society in his *résumé* of Sig. Schiaparelli's labours alludes to the classes of celestial matter as follows:—"1st. Fixed stars. 2nd. Agglomerations of small stars (resolvable *nebulæ*). 3rd. Smaller bodies, invisible, except when approaching the sun. 4th. Small particles composing a cosmical cloud."

"Whatever may have been the original form of the cloud, it cannot penetrate far into our system without assuming the form of an elongated cylinder passing gradually into a stream of particles. The number of such streams seems to be very great." Such are the views of the medallist; and of these views the late President expresses his opinion in the following words:—"Granting that his hypotheses are correct, of which, indeed, there seems to be a very high probability, some of the most difficult questions in the contemplation of the constitution of the universe seem at once, and, as it were, *per saltum*, to be solved. To have placed before our view so clear a history of these mysterious bodies (*nebulæ*, comets and *aerolites*), and their several and intimate relations pointed out, is an advancement of astronomical science I at least, individually, had not ventured to anticipate; and a collateral advantage resulting from this splendid discovery is the encouragement given to the careful and diligent observation of phenomena even when the prospect of a fruitful result is by no means apparent; had it not been for the patient, systematic, and intelligent observations of Professor Heis, M. Coulvier-Gravier, Mr. Greg, and Professor Herschel, Signor Schiaparelli would have wanted many valuable data required in his investigations."

In connexion with the splendid discovery of Schiaparelli, Mr. Williams's book must become very valuable. The arrangement of the catalogue of comets is so methodical, so clear, and the dates given with such precision, that the identification of future comets with ancient ones will be greatly facilitated. Although the work is necessarily destitute of computed orbits, the paths of many of the comets amongst the stars are fully described and capable of being easily ascertained by the aid of the Chinese celestial atlas with which it is accompanied. Our limits preclude further remarks, except that Mr. Williams has introduced a most interesting description of ancient Chinese astronomy, as well as an important exposition of Chinese chronology.

*Weather Charts issued daily by the Meteorological Office.*

These charts, which are admirably arranged, exhibit at a glance the general elements of weather at 8 A.M. each day; these elements consist of barometric and thermometric readings depicted as isobars and isotherms, the general direction of the wind, the state of the



sea, and the prevalence or otherwise of clouds and rain. The land areas are readily distinguishable by being shaded. If we are not greatly mistaken, the maps of the barometer and wind will prove to be the most valuable portion; but we greatly regret that the area over which the elements are given is so small that by our instruments we can only obtain fragments of them; and unless the meteorologist combines the data given in these charts with other data obtainable on the continent or ocean, we apprehend his work will not be easy from the charts before us to obtain a general view of the affections of the atmosphere as they exist in their entirety.

*An Elementary Treatise on the Differential Calculus, containing the Theory of Plane Curves, with numerous Examples.* By BENJAMIN WILLIAMSON, A.M., Fellow and Tutor, Trinity College, Dublin. London: Longmans, Green, and Co. 1872. (Pp. 343.)

The various English Treatises on the Differential Calculus so closely resemble each other as to the subjects of their successive chapters, that we shall sufficiently describe the scope of the present volume by saying that it comprises all the parts of the subject commonly treated. For instance, if the table of contents be compared with that of Mr. Todhunter's well-known treatise, they will be found to differ simply in arrangement. We do not mean to imply that the resemblance extends beyond this point. On the contrary, Mr. Williamson's Treatise is as completely his own as any book on a well-worked subject can be an author's own. In fact, so far as treatment is concerned, these two authors take a fundamentally different view. The one says:—"My own experience with pupils has been decidedly unfavourable to the system of Differentials; many successful teachers whom I have consulted have expressed a similar opinion; and I have therefore adopted exclusively the method of Differential Coefficients" (Pref. to 3rd edit.). Mr. Williamson, on the other hand, says:—"An exclusive adherence to the method of Differential Coefficients is by no means necessary for clearness and simplicity; and, indeed, I have found by experience that many fundamental investigations in mechanics and geometry are made more intelligible to beginners by the method of differentials than by that of differential coefficients." We shall not attempt to decide between the authors; but this at least may be said, that Mr. Williamson has proved by example that a most instructive and useful book can be produced, if written from his point of view. It is perhaps in the Geometrical applications of the Calculus that the best features of the book are to be seen: *e. g.* the illustration of partial differentiation by discussing the variations of the parts of plane and spherical triangles (pp. 110-118) is very happy, and the chapters on Tangents, Asymptotes, Multiple points, Inflection, and Curvature are exceedingly well done; they have been written with Dr. Salmon's work on "Higher Plane Curves" in view, and contain several points which are not generally given in the corresponding chapters of other elementary treatises, such as the notice of Inverse curves, Pedal curves, the introductory notice on Multiple points, &c. Very

many examples are given; these are needed in an elementary treatise, to render the work independent of a separate volume of examples. So far as we have examined them they seem well chosen, and in many cases are, in fact, interesting theorems:—*e. g.*, “Prove that the points of intersection of a curve of the fourth degree with its asymptotes lie on a conic;” and “Prove that every curve of the third degree is capable of being projected into a central curve,” &c.

*New Theory of the Figure of the Earth, considered as a Solid of Revolution; founded on the direct employment of the Centrifugal Force instead of the Common Principles of Attraction and Variable Density.* By WILLIAM OGILBY, Esq., M.A. Trin. Coll. Camb., Fellow of the Geological and Zoological Societies of London and Dublin, Member of the Royal Irish Academy, &c. London: Longmans, Green, and Co. 1872. (4to, pp. 104.)

Mr. Ogilby in his preface enumerates seven “principal fallacies and hypotheses invoked in this artificial [*i. e.* the usual] mode of treatment” of the question of the figure of the earth. The first two are these:—“*First*, while it is universally acknowledged that the spheroidal figure is due to the action of the centrifugal force, the efficient cause of the phenomenon is sedulously ignored in the investigation of its effects, and the question treated on the principle of attraction, as if it were a case of elliptic motion. *Secondly*, this pretended attraction, though professing to be the resultant of the individual attractions of all the particles, must not be confounded with terrestrial gravity; inasmuch as gravity varies *directly* as the square of the sine of the latitude, acts in the normal, and is consequently directed to a different centre at every point on the surface, while the assumed attraction tends to a fixed centre of force, and varies *inversely* as the square of the distance &c.” (p. viii). We should be sorry to hold Mr. Ogilby tightly to what he says; but if we did, it would follow that the force of gravity at the equator is zero. Anyhow it is pretty plain from the above extract that he is not a believer in universal gravitation; nor, indeed, if he is not satisfied with the evidence, is there any reason why he should be; only, if so, it is a little strange that his object is to employ mathematical reasoning “in imitation of Newton” (p. xii), and that, with reference to applied science, he should speak of Newton as “the great master” (p. 2). The chief offender in this matter of the figure of the earth is Laplace, who says that the law of gravity on the earth’s surface depends on the form of the terrestrial spheroid, and this in turn on the law of gravity, and that this mutual dependence of the two unknown quantities renders the investigation of the figure of the earth very difficult. This is no more than might have been expected from Laplace; but that in writing thus he should only be following up a line of inquiry begun by Newton demands explanation; and here is the explanation supplied by Mr. Ogilby (p. 14):—“It is difficult to account for all these arbitrary assumptions, unless by supposing that the mind of Newton was so completely absorbed in his grand

discovery, that he could recognize no other principle than attraction in the regulation of phenomena." These words refer to Prop. XIX. of Book 3 of the *Principia*, which contains what Mr. Airy calls "this astonishing investigation," adding that "it is one of the many instances in which Newton has obtained a correct result by means apparently quite inadequate." This seems strange. It may of course be that Newton and Laplace are both wrong; but then it may be that Mr. Ogilby is wrong. Suppose, then, we try to ascertain his qualifications for discussing a difficult question by seeing how he deals with a comparatively simple one. Here, for instance, is his notion of the effect of "the centrifugal force" on a fluid in a state of rotation. "The centrifugal force generated by rotation would repel the fluid particles from the interior parts and leave a vacant ellipsoidal space round the axis, of greater or less extent, according to the velocity of rotation and the density of the fluid mass" (p. 97); from this he argues that the hypothesis of the original fluidity of the earth is inconsistent with itself. However this may be, it must at least be allowed that according to this beautiful conception a cylinder of the fluid coinciding in direction with the axis of revolution would be supported by nothing, and that this is a view of the effects of rotation which will perhaps find acceptance when the views of Newton and Laplace are forgotten.

Here is another instance of Mr. Ogilby's powers. It must be premised that gravity at the equator is to be represented by 288 and at the pole by 289, or more generally by  $m$  and  $m+1$ . By a process of reasoning he finds (p. 53) that the following relation exists between these numbers,

$$\frac{1}{2}(m+1)^4 + \frac{1}{2}m^4 = m^2 \times (m+1)^2.$$

That there may be no mistake as to his meaning, he enunciates this equation in words printed in italics, and goes on to state, "this theorem enables us to correct errors both of astronomical and geodetical observation in measuring the ellipticities of the earth and planets." The reader will notice that the above equation may be written thus:—

$$\{(m+1)^2 - m^2\}^2 = 0,$$

and that this gives, as alternative results,

$$1 = 0 \text{ or } 577 = 0.$$

Need we add that the treatise has been written in the interests of true religion (pp. xii, 50, 51, 103, 104)?

We have spent a good deal of time on this treatise, and have got little profit from it; for we are not psychologists. Had it been otherwise, it would have suggested many curious questions as to possible types of mind and character, and we should probably have ended our study of the book by classifying its author with the squarers of the circle.



XXXVIII. *Proceedings of Learned Societies.*

## ROYAL SOCIETY.

[Continued from p. 234.]

Nov. 23, 1871.—General Sir Edward Sabine, K.C.B., President, followed by Mr. Francis Galton, Vice-President, in the Chair.

THE following communication was read:—

“On a supposed Alteration in the amount of Astronomical Aberration of Light, produced by the passage of Light through a considerable thickness of Refracting Medium.” By George Biddell Airy, C.B., Astronomer Royal.

A discussion has taken place on the Continent, conducted partly in the ‘*Astronomische Nachrichten*,’ partly in independent pamphlets, on the change of direction which a ray of light will receive (as inferred from the Undulatory Theory of Light) when it traverses a refracting medium which has a motion of translation. The subject to which attention is particularly called is the effect that will be produced on the apparent amount of that angular displacement of a star or planet which is caused by the Earth’s motion of translation, and is known as the Aberration of Light. It has been conceived that there may be a difference in the amounts of this displacement as seen with different telescopes, depending on the difference in the thicknesses of their object-glasses. The most important of the papers containing this discussion are:—that of Professor Klinkerfues, contained in a pamphlet published at Leipzig in 1867, August; and those of M. Hoek, one published 1867, October, in No. 1669 of the ‘*Astronomische Nachrichten*,’ and the other published in 1869 in a communication to the Netherlands Royal Academy of Sciences. Professor Klinkerfues maintained that, as a necessary result of the Undulatory Theory, the amount of Aberration would be increased, in accordance with a formula which he has given; and he supported it by the following experiment:—

In the telescope of a transit-instrument, whose focal length was about 18 inches, was inserted a column of water 8 inches in length, carried in a tube whose ends were closed with glass plates; and with this instrument he observed the transit of the Sun, and the transits of certain stars whose north-polar distances were nearly the same as that of the Sun, and which passed the meridian nearly at midnight. In these relative positions, the difference between the Apparent Right Ascension of the Sun and those of the stars is affected by double the coefficient of Aberration; and the merely astronomical circumstances are extremely favourable for the accurate testing of the theory. Professor Klinkerfues had computed that the effect of the 8-inch column of water and of a prism in the interior of the telescope would be to increase the coefficient of Aberration by eight seconds of arc. The observation appeared to show that

the Aberration was really increased by  $7''\cdot1$ . It does not appear that this observation was repeated.

A result of physical character so important, and resting on the respectable authority of Professor Klinkerfues, merited and, indeed, required further examination. Having carefully considered the astronomical means which would be most accurately employed for the experiment, I decided on adopting a vertical telescope, the subject of observation being the meridional zenith distance of  $\gamma$  Draconis, the same star by which the existence and laws of Aberration were first established. The position of this star is at present somewhat more favourable than it was in the time of Bradley, its mean zenith-distance north at the Royal Observatory being about  $100''$  and still slowly diminishing. With the sanction of the Government, therefore, I planned an instrument, of which the essential part is that the whole tube, from the lower surface of the object-glass to a plane glass closing the lower end of the tube, is filled with water, the length of the column of water being  $35\cdot3$  inches. The curvatures of the surfaces of the two lenses constituting the object-glass, adapted, in conjunction with the water, to correct spherical and chromatic aberration, were investigated by myself and verified by my friend Mr. Stone (now Astronomer at the Cape Observatory). The micrometer is constructed on a plan arranged by myself, by which the double observation in reversed positions of the instrument can be made with great ease. The reference to the vertical is given by two spirit-levels, both to be read at every single observation. The work of construction was intrusted to Mr. James Simms, who carried it out with great ability. Distilled water was supplied by H.W. Chisholm, Esq., Warden of Standards.

Had the result of the observations been confined to the determination of an astronomical constant, or the variation of its value for different telescopes, I should not have thought it worthy of communication to the Royal Society. But it is really a result of great physical importance, not only affecting the computation of the velocity of light, but also influencing the whole treatment of the Undulatory Theory of Light. In this view I have thought that an informal statement of the conclusions may be acceptable to the Society, reserving for publication in one of the annual Greenwich Volumes the details of the observations.

The instrument was mounted in a small Occasional Observatory first constructed for the transit-instrument of Mr. Struve when he was engaged in determining the longitude of Altona, and now planted on the "South Ground" of the Observatory. The seasons at which the meridional zenith-distance of  $\gamma$  Draconis is most affected by aberration in opposite directions are the Equinoxes.

For understanding the following Table, it is to be remarked that an apparent value of the Geographical Latitude of the Instrument is formed from every observation, by subtracting the Observed Instrumental Zenith-distance North of the Star from the Tabular Declination of the Star given in the 'Nautical Almanac.' The observed zenith-distance is affected with the True Aberration as seen in the

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instrument; the tabular declination is affected with the Received Aberration used in the computation of the 'Nautical Almanac;' and the apparent value of the geographical latitude is therefore affected by the difference between the True Aberration as seen in the instrument and the Received Aberration. If, therefore, under all circumstances, and especially in the comparison of days when the sign of aberration has changed, the apparent value of the geographical latitude is sensibly constant, it proves that the True Aberration is the same as the Received Aberration, or at least that one is not a multiple of the other.

The last column of the Table is given only to show to how large an extent Aberration enters into the star's Apparent Declination.

Every result for Observed Zenith-distance in the Table is the mean of observations in reversed positions of the instrument.

Day of observation.	Star's Observed Zenith-distance North.	Star's Declination from 'Nautical Almanac.'	Difference for Geographical Latitude of Instrument.	Correction for Aberration adopted in 'Nautical Almanac.'
1871.				
Feb. 28 .....	85° 30	51° 29' 59.3	51° 28' 34.0	-18.71
March 1 .....	85° 71	59.1	33.4	18.82
3 .....	84° 19	58.9	34.7	19.02
4 .....	82° 18	58.8	36.6	19.11
16 .....	83° 63	58.0	34.4	19.73
17 .....	84° 58	58.0	33.4	19.74
21 .....	83° 87	57.9	34.0	19.73
23 .....	82° 73	57.9	35.2	19.69
24 .....	84° 18	58.0	33.8	19.66
26 .....	84° 04	58.1	34.1	19.59
27 .....	83° 48	51° 29' 58.2	51° 28' 34.7	-19.54
Mean Latitude of Instrument from Spring Observations .....			51° 28' 34.4	
Aug. 29 .....	122° 10	51° 30' 34.4	51° 28' 32.3	+18.25
Sept. 5 .....	121° 84	35.0	33.2	19.01
7 .....	121° 62	35.1	33.5	19.18
9 .....	120° 27	35.2	34.9	19.33
11 .....	122° 98	35.3	32.3	19.45
15 .....	122° 20	35.4	33.2	19.64
17 .....	121° 53	35.5	34.0	19.70
22 .....	121° 38	35.5	34.1	19.74
24 .....	120° 01	35.4	35.4	19.72
Oct. 1 .....	120° 62	35.1	34.5	19.46
2 .....	120° 29	35.1	34.8	19.40
3 .....	121° 31	35.0	33.7	19.33
4 .....	124° 41	34.9	30.5	19.26
6 .....	120° 60	51° 30' 34.8	51° 28' 34.2	+19.10
Mean Latitude of Instrument from Autumn Observations .....			51° 28' 33.6	



Remarking that the mean results for Geographical Latitude of the Instrument (determined from observations made when the Aberration of the star had respectively its largest + value and its largest - value) agree within a fraction of a second, I think myself justified in concluding that the hypothesis of Professor Klinkerfues is untenable. Had it been retained, the Aberrations to be employed in the corrections would have been increased by + 15" and - 15" respectively, and the two mean results would have disagreed by 30".

The latitude of the instrument from these observations is about  $51^{\circ} 28' 34''.0$ . The position of the instrument, as measured on the Observatory Map, is 340 feet south of the Transit-circle, a spatial distance corresponding to about  $3''.35$ . The latitude of the Transit-circle being taken at  $51^{\circ} 28' 38''.4$ , the geodetic latitude of the instrument is  $51^{\circ} 28' 35''.05$ —an agreement closer than I expected, consideration being given to the form of the ground. It appears very probable that at the place of the Transit-circle, on the north brow of the hill, the zenithal direction is disturbed towards the north, and the astronomical latitude is too great.

There is only one point in this investigation upon which a doubt can be suggested as possible, namely the evaluation of the micrometer-scale. It was thus conducted:—The micrometer-plate contains 26 wires, and the fixed part of the instrument contains 25 crosses, each interval being nearly  $256''$ . With this arrangement every wire-interval is measured with great ease, and the whole series of 25 intervals is accurately obtained in terms of the micrometer. By placing the instrument in a proper position, the same intervals are obtained in time of the star's transit, which is easily converted into arc. The comparison of these gives the value of micrometer-divisions which has been employed.

The following verification, of somewhat inferior accuracy, has been made by measures of the instrument. It appears that the ray of light passes through 0.9 inch of glass, 35.3 inches of water, and 0.8 inch of air (nearly, the measure of the last being slightly uncertain). Remarking that the dividing surfaces are horizontal and plane, it is easily seen that the micrometer-scale ought to be such as is due to an air-telescope whose length in inches  $= \frac{0.9}{1.6} + \frac{35.3}{1.336} + 0.8 = 27.8$  inches. And from this, with observation of transit of the star, it was found that the measure of 25 intervals of wires ought to be 0.8693 inch: as measured with a pair of compasses, it was found sometimes 0.871, sometimes 0.875. The agreement is fully as close as can be expected from the rudeness of the operation, and shows distinctly that there can be no error of principle in the method of evaluating the micrometer-scale.

## GEOLOGICAL SOCIETY.

[Continued from p. 238.]

December 20th, 1871.—Joseph Prestwich, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. A Letter from G. Milner Stephen, Esq., F.G.S., to the late Sir Roderick Murchison, dated Sydney, 5 October, 1871, announcing the discovery of a rich auriferous deposit on the banks of the river Bondé, on the N.E. coast of New Caledonia, and of a great deposit of Tin-ore in the district of New England, New South Wales. The gold in New Caledonia is found in drift; and there are indications of the near proximity of a quartz-reef. The Tin-ore in New South Wales is said to be in "pepitas, crystals, and beds of conglomerate, especially in Micaceous granite, more or less decomposed."

2. "Remarks on the Greenland Meteorites." By Prof. A. E. Nordenskjöld, For. Corr. G.S.

The author stated that the masses of meteoric iron brought from Greenland by the recent Swedish expedition seem to have formed the principal masses of an enormous meteoric fall of Miocene date, extending over an area of some 200 miles. The iron appears to be free from silicates. Against its eruptive origin the author urges that when heated it evolves a great amount of gaseous matter, and that it contains imbedded particles of sulphide of iron, the mass itself being nearly free from sulphur. The masses are composed of meteoric nickeliferous cast and wrought iron, or of mixtures of the two; in the last case the Widmannstätten's figures are best developed. The author further noticed the various modes in which the iron occurs, viz.:—1, as meteorites; 2, filling cracks; 3, as breccia-form stones cemented with oxide and silicate of iron; and 4, in grains disseminated in the basalt.

3. "Further Remarks on the Relationship of the *Limulidæ* (*Xiphosura*) to the *Eurypteridæ* and to the *Trilobita*." By Henry Woodward, Esq., F.G.S.

In this paper the author described the recent investigations made by Dr. A. S. Packard, Dr. Anton Dohrn, and the Rev. Samuel Lockwood, upon the developmental history of the North-American King-crab (*Limulus Polyphemus*), and discussed the conclusions as to the alliances of the Xiphosura and Eurypteridæ, and to the general classification of the Arthropoda, to which the results of these investigations have led Dr. Dohrn and some other continental naturalists. According to this view, the Xiphosura and Eurypteridæ are more nearly related to certain Arachnida (the Scorpions, &c.) than to the Crustacea; and this opinion is further supported by the assertion of Dr. Dohrn, that in *Limulus* only one pair of organs (antennules) receives its nerves from the supracerebral ganglion, and that the

nature of the under lip in *Limulus* differs from that prevailing among the Crustacea. Dr. Dohrn also recognizes the relationship of the Merostomata to the Trilobites, as shown especially by the development of *Limulus*, and considers that the three forms (*Limulidæ*, *Eurypteridæ*, and *Trilobitæ*) should be combined in one group under the name of *Gigantostroma*, proposed by Hæckel, and placed beside the Crustacea. The author stated, on the authority of Prof. Owen, that *Limulus* really possesses two pairs of appendages which receive their nerves from the supracæsophageal ganglion, that, according to Dr. Packard, the young *Limulus* passes through a Nauplius-stage while in the egg, that no argument could be founded upon the lower lip, the condition of which varied extremely in the three groups proposed to be removed from the Crustacea; and he maintained that, even from the ultra-Darwinian point of view taken by Dr. Dohrn, the adoption of his proposal would be fatal to the application of the hypothesis of evolution to the class Crustacea.

January 10, 1872.—Joseph Prestwich, Esq., F.R.S., President,  
in the Chair.

The following communications were read:—

1. "On *Cyclostigma*, *Lepidodendron*, and *Knorria* from Kiltorkan." By Prof. Oswald Heer, F.C.G.S.

In this paper the author indicated the characters of certain fossils from the Yellow Sandstone of the South of Ireland, referred by him to the above genera, and mentioned in his paper "On the Carboniferous Flora of Bear Island," read before the Society on November 9, 1870 (see Q. J. G. S. vol. xxvii. p. 1). He distinguished as species *Cyclostigma kiltorkense*, Haught., *C. minutum* (Haught.), *Knorria acicularis*, Göpp., var. *Bailyana*, and *Lepidodendron Veltheimianum*, Sternb.

2. "Notes on the Geology of the Plain of Marocco, and the Great Atlas." By George Maw, Esq., F.G.S. &c.

The author described first the characters presented by the coast of Marocco, and then the phenomena observed by him in his progress into the interior of the country and in the Atlas Chain. The oldest rocks observed were ranges of metamorphic rocks bounding the plain of Marocco, interbedded porphyrites and the porphyritic tuffs forming the backbone of the Atlas Chain, and the Mica-schists of Djeb Tezah in the Atlas. At many points in the lateral valleys of the Atlas almost vertical grey shales were crossed; the age of these was unknown. Above these comes a Red Sandstone and Limestone series, believed to be of Cretaceous age, and beds possibly of Miocene age, which occupied the valleys of the Atlas and covered the plain of Marocco, where vestiges of them remain in the form of tubular hills. The probable age of these beds was determined on the evidence of fossils. The author noticed the sequence of denuding and eruptive phenomena by which the arrangement and distribution of these rocks has been modified, and described the more recent changes—resulting in the formation of enormous boulder-beds flanking the



northern escarpment of the Atlas plateau, and of great moraines at the heads of the valleys of the Atlas, both of which he ascribed to glacial action. An elevation of the coast line of at least 70 feet was indicated by raised beaches of concrete sand at Mogador and elsewhere; and the author considered that a slight subsidence of the coast was now taking place. The surface of the plain of Marocco was described as covered with a tufaceous crust, probably due to the drawing up of water to the surface from the subjacent calcareous strata and the deposition from it of laminated carbonate of lime.

### XXXIX. *Intelligence and Miscellaneous Articles.*

AN EXPERIMENT IN REFERENCE TO THE QUESTION AS TO VAPOUR-VESICLES. BY T. PLATEAU.

FROM a research of M. Duprez\*, it is known that when a vessel of water is inverted with the opening downwards, it is not necessary that this opening be very narrow for the water to remain suspended. M. Duprez kept water suspended in a vertical tube which was nearly 20 millims. in internal diameter.

Assuming now that when water is hanging in such a tube with the surface downwards a small hollow air-bubble is brought into contact with this surface, the air in it, in virtue of the pressure of the envelope, will immediately penetrate into the interior of the liquid, and will rise in it in virtue of the smaller specific gravity. This I have confirmed by means of an experiment. I took a small glass tube of about 4 millims. diameter in the clear, drew it out at one end to an aperture of about 0.4 millim. diameter, and closed the wider end by a cork coated with grease. By touching the drawn-out end with a piece of filtering paper, which was soaked with distilled water, I succeeded in bringing into the narrow aperture a column of this liquid not more than a millimetre in length. By carefully depressing the cork, a hollow bubble is seen to form at the drawn-out aperture, which may have a diameter of less than a millimetre, and usually lasts seven or eight seconds. In this operation the wider part of the tube must be covered with several layers of a non-conducting material in order to eliminate the influence of the warmth of the fingers. After having thus acquired the power of procuring very small hollow bubbles, water was suspended in a tube kept vertical by a suitable stand. The internal diameter of this tube was only a centimetre; and with such a small diameter the suspension is very easy. It is only necessary, after filling the tube with water and closing the mouth with a piece of paper, to invert it and then draw the paper aside to get a free surface; a hollow water bubble of less than a millimetre diameter is then produced as described above, and brought to the free surface of the suspended water. When they are in contact, the bubble detaches itself from the drawn-

\* "Mém. sur un cas particulier de l'équilibre des liquides," *Mém. de l'Académie Roy. de Belgique*, vol. xxvi. 1851, and xxviii. 1854.

out tube; the air contained in it penetrates and ascends in the liquid. The experiment, repeated several times, always gave the same result.

Let us now assume that at a certain distance below the surface of the suspended water there is a current of visible aqueous vapour; if this vapour consists of vesicles, each of them, on coming into contact with the fluid surface, will introduce a microscopic air-bubble into the water, which will immediately ascend in it; and the whole of these vesicles will form a cloud in the water of the tube, which slowly rises and destroys the transparency.

M. Duprez was good enough to make the experiment at my request. The water was suspended in a glass tube of 13 millims. internal diameter. A small metal vessel with an aperture of several centimetres diameter and containing a certain quantity of water, was placed under the free surface of the water of the tube over a lamp; the mouth of this vessel was about 12 centims. from the surface. A continuous boiling was thus obtained, and a current of visible vapour which rose to the surface of the suspended water; but though the experiment lasted more than half an hour, no cloud was observed in the water of the tube. The vapour condensed on the outside of the tube, and from time to time was wiped away; but the water inside retained all its transparency.

After this it seems difficult to retain any doubt as to the non-existence of the vesicular state. It seems to me, in fact, that only three objections could here be raised. It might either be said that the air-bubbles on penetrating into the water, from their unusual smallness and the very considerable capillary pressure which they have to support from the liquid, dissolve in the liquid, or that all vesicles burst on reaching the surface of the water, or that they roll along the surface of this liquid, separated from it by a thin layer of air or vapour, until they reach the outside edge of the tube to escape thence into the atmosphere.

But the first of these assumptions must be rejected; for the water had previously been shaken with air so long as to be completely saturated, and, secondly, while it was exposed to the action of vapour it must have lost whatever solvent power it possessed; and, moreover, sometimes even comparatively large air-bubbles appear on the upper part of the inside of the tube, where the hotter part of the water ascends.

The second supposition, though not quite inadmissible, is at any rate not very probable. We have seen that our small bubbles, measuring less than a millimetre, do not burst on coming into contact with the surface of water; why should it be otherwise with the vesicles? It may perhaps, be urged that their envelope is far thinner than that of our small bubbles. But if vesicles exist, their envelope must be so thick that they are colourless; otherwise a cloud irradiated by the sun could have no bright lustre; moreover, from the long duration of large clouds, they must be very permanent.

As regards the third supposition, is it probable that all vesicles could roll along the surface without touching? Moreover M. Duprez

has repeated the experiment in such a manner that this surface was concave and remained so in spite of the fact that the volume of water increased owing to expansion by heat and the condensation of vapour; but now in this case a large number of vesicles should have rolled towards the apex of the concavity, have accumulated there, and therefore must soon have placed themselves in contact with the fluid surface: but nothing in the result was different; no cloud disturbed the transparency of the water.

I consider the above experiment, though not decisive, yet a very powerful argument against the hypothesis of the vesicular condition.

May I here be permitted to recall another experiment, which I have described in the eighth series of my investigation *Sur les figures d'équilibre d'une masse liquide sans pesanteur*. One of the chief objections which have been raised against the vesicular condition is, that the air contained in a vesicle would be exposed on the part of the fluid envelope to a considerable pressure, the effect of which would be that this air would gradually dissolve in the envelope, and would traverse it, by which the vesicle would soon change into a complete globule. But when a laminar calotte, about a centimetre in diameter, is formed by means of a solution of 1 part of Marseilles soap in 40 of water, and this is allowed to stand in an atmosphere laden with aqueous vapour, it sometimes lasts for more than twenty-four hours and becomes quite black. At the same time a remarkable phenomenon is witnessed; the calotte gradually decreases and ultimately disappears,—from which it follows that the enclosed air has gradually traversed the lamina. This lamina is indeed far thinner than a vesicle would be; but, on the other hand, theory shows, from the difference of the liquids and the diameter, that in the interior of a vesicle the pressure would be more than a thousand times as great as in the interior of a calotte of soap-solution at its original dimensions.—*Bull. de l'Acad. Roy. de Belgique*, vol. xxxii.

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ON THE ABSORPTION-SPECTRA OF CHLORINE AND CHLORIDE OF IODINE. BY D. GERNEZ.

The researches of Brewster, of W. H. Miller, and of W. A. Miller have made known the absorption-spectra of the coloured vapours of hyponitric acid, bromine, iodine, hypochloric and chlorous acids, and perchloride of manganese. These vapours act very energetically on light; and in a thickness of only a few centimetres they produce characteristic systems of lines in the continuous spectra of incandescent solids.

When, with the aid of a spectroscope with only one prism, we examine the effect produced by an increasing thickness of these substances, we first observe some bands, the number of which increases while finer lines appear and the primitive bands are resolved into groups of very close lines. It is these bands, or rather the most conspicuous of the lines, that we again meet with, as I have recently shown, in the spectra of solutions of these substances. If we aug-



ment the dispersion of the luminous pencil at its issue from the liquid, the bands spread over a larger surface, and lose their intensity ; so that we gain nothing for the observation of the absorption-spectrum of a liquid by using a spectroscope with several prisms. It is not the same when we have to do with vapours, almost all the lines of which are very fine, and appear more distinctly the more the luminous pencil is spread out and the source whence it emanates is more intense.

This remark led me to recognize the existence of the absorption-spectra of chlorine and chloride of iodine. In the examination of that of chlorine, I had to take particular care to operate on pure gas. In fact, the phenomenon being insensible for a thickness of some decimetres, as the fruitless endeavours of some physicists have shown, it was necessary to employ a gaseous column of greater dimensions ; and as very small quantities of chlorous and hypochloric acids were sufficient to exhibit the absorption-lines of these substances, the impurities of the gas prepared with hydrochloric acid and binoxide of manganese might, in the conditions of the experiment, have caused the appearance of the absorption-lines of the chlorinated compounds. To destroy these products, which do not resist the action of heat, I passed the dry chlorine through a glass tube heated to redness.

On causing the light to pass along the axis of a tube of 15 decims. length filled, under the atmospheric pressure, with chlorine thus purified, I could distinguish very clearly the absorption-lines of this gas ; but the phenomenon is more striking in an apparatus of greater length. The one I made use of was composed of three glass collars 6 centimetres in diameter, adjusted end to end so as to form a tube of 468 decims. length ; this was closed at its two extremities with parallel disks of plate-glass. Having placed it erect in order to fill it with chlorine by bringing the gas through a tube to its lower part while the air escaped through an aperture made at its upper extremity, I placed it horizontal and caused to pass along its axis a pencil of the Drummond light. On emerging from the gas, the rays fell on the slit of a spectroscope with two prisms, and gave a spectrum extending into the violet and marked with very distinct lines. In the least-refrangible region and as far as the place of the line D the spectrum is continuous ; but a little beyond that a system of lines commences which presents on analogy with the fine, almost equidistant lines observed in the vapours of bromine and iodine. Their aspect and intensity vary with the region of the spectrum examined ; and they extend nearly to the violet, which is entirely absorbed in the case of the luminous source in question.

The protochloride of iodine is much more favourable than chlorine for the observation of the lines of absorption. At the temperature of  $40^{\circ}$  C., a thickness of 30 centims. of this substance furnishes sufficient vapour to produce an absorption-spectrum composed of a score of fine lines, sensibly equal in intensity, their distance diminishing very little from the extreme red, where they commence, to a little beyond the line D, where they end ; two other lines, rather intense,

appear in the yellow ; and no others are distinguished in the rest of the spectrum.

This system of lines, very different from that of chlorine, is analogous to those of bromine and iodine ; but it differs from that of iodine by the absence of bands superposed to the fine lines in the green, and also because the lines of chloride of iodine begin to show themselves notably nearer the extreme red than do those of iodine, and cover only a much less extensive region of the spectrum.—*Comptes Rendus de l'Acad. des Sci.* 1872, No. 10.

ON THE MEAN MOTIONS OF JUPITER, SATURN, URANUS, AND NEPTUNE. BY PROFESSOR DANIEL KIRKWOOD.

The recent note of Professor Peirce\*, announcing his discovery of some interesting relations between the mean motions of the four outer planets, has recalled my attention to a number of similar coincidences detected by myself several years since, while engaged in a somewhat laborious examination of the planetary elements. Of these, the following may be worth putting on record for future discussion :—

$$2n^v - 3n^{vi} - 11n^{viii} = 0, \dots\dots\dots (1)$$

$$2n^{vi} - 21n^{vii} + 30n^{viii} = 0, \dots\dots\dots :.. (2)$$

$$3n^v - 8n^{vi} - 2n^{vii} + 7n^{viii} = 0. \dots\dots\dots (3)$$

With the values of  $n^v$ ,  $n^{vi}$ , and  $n^{vii}$  adopted in the American Ephemeris, the value of  $n^{viii}$  obtained from either of the above equations differs by less than one second from the latest determination†. The second equation was submitted some two years since to Professor Newcomb of the U.S. Coast Survey. That distinguished astronomer was inclined, however, to regard the coincidence as merely accidental. Be this as it may, I have strong confidence in the accuracy of the third. The reexamination of this last has recently led to the discovery of two others, viz.

$$68n^{vi} - 425n^{vii} + 257n^{viii} = 0, \dots\dots\dots (4)$$

$$257n^v - 844n^{vi} + 587n^{vii} = 0. \dots\dots\dots (5)$$

both of which, I believe, are accurately true. The fifth, however, is not an independent equation, but is derived from the third and fourth. By means of these equations I have found the remarkable cycle of 11657.24 Julian years, which separates the epochs at which the planets Jupiter, Saturn, Uranus, and Neptune return to the same relative positions. It is obvious, moreover, from the same equations that no three of the four outer planets can ever be in conjunction at the same time.—Silliman's *American Journal* for March 1872.

\* Silliman's *Journal* for January 1872. It will be observed that Professor Peirce's third equation is identical with that discovered by Professor Newcomb in 1857. See Gould's *Astr. Jour.* vol. v. p. 101.

† Newcomb's 'Orbit of Neptune,' p. 76.

THE  
LONDON, EDINBURGH, AND DUBLIN  
PHILOSOPHICAL MAGAZINE  
AND  
JOURNAL OF SCIENCE.

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[FOURTH SERIES.]

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MAY 1872.

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XL. *On the Reflection and Refraction of Light by intensely Opaque Matter.* By the Hon. J. W. STRUTT, M.A., late Fellow of Trinity College, Cambridge\*.

IT is, I believe, the common opinion, that a satisfactory mechanical theory of the reflection of light from metallic surfaces has been given by Cauchy, and that his formulæ agree very well with observation. The result, however, of a recent examination of the subject has been to convince me that, at least in the case of vibrations performed in the plane of incidence, his theory is erroneous, and that the correspondence with fact claimed for it is illusory, and rests on the assumption of inadmissible values for the arbitrary constants. Cauchy, after his manner, never published any investigation of his formulæ, but contented himself with a statement of the results and of the principles from which he started. The intermediate steps, however, have been given very concisely and with a command of analysis by Eisenlohr (*Pogg. Ann.* vol. civ. p. 368), who has also endeavoured to determine the constants by a comparison with measurements made by Jamin. I propose in the present communication to examine the theory of reflection from thick metallic plates, and then to make some remarks on the action on light of a *thin* metallic layer, a subject which has been treated experimentally by Quincke.

The peculiarity in the behaviour of metals towards light is supposed by Cauchy to lie in their *opacity*, which has the effect of stopping a train of waves before they can proceed for more

\* Communicated by the Author.



than a few wave-lengths within the medium. There can be little doubt that in this Cauchy was perfectly right; for it has been found that bodies which, like many of the dyes, exercise a very intense selective absorption on light, reflect from their surfaces in excessive proportion just those rays to which they are most opaque. Permanganate of potash is a beautiful example of this, given by Professor Stokes. He found (Phil. Mag. vol. vi. p. 293) that when the light reflected from a crystal at the polarizing angle is examined through a Nicol held so as to extinguish the rays polarized in the plane of incidence, the residual light is green, and that, when analyzed by the prism, it shows bright bands just where the absorption-spectrum shows dark ones. This very instructive experiment can be repeated with ease by using sunlight, and instead of a crystal a piece of ground glass sprinkled with a little of the powdered salt, which is then well rubbed in and burnished with a glass stopper or otherwise. It can without difficulty be so arranged that the two spectra are seen from the same slit one over the other, and compared with accuracy.

With regard to the chromatic variations, it would have seemed most natural to suppose that the opacity may vary in an arbitrary manner with the wave-length, while the optical density (on which alone in ordinary cases the refraction depends) remains constant, or is subject only to the same sort of variations as occur in transparent media. But the aspect of the question has been materially changed by the observations of Christiansen and Kundt (Pogg. *Ann.* vols. cxli., cxliii., cxliv.) on anomalous dispersion in Fuchsin and other colouring-matters, which show that on either side of an absorption-band there is an abnormal change in the refrangibility (as determined by prismatic deviation) of such a kind that the refraction is *increased* below (that is, on the red side of) the band and *diminished* above it. An analogy may be traced here with the repulsion between two periods which frequently occurs in vibrating systems. The effect of a pendulum suspended from a body subject to horizontal vibration is to increase or diminish the virtual inertia of the mass according as the natural period of the pendulum is shorter or longer than that of its point of suspension. This may be expressed by saying that if the point of support tends to vibrate more rapidly than the pendulum, it is made to go faster still, and *vice versâ*. Below the absorption-band the material vibration is naturally the higher, and hence the effect of the associated matter is to increase (abnormally) the virtual inertia of the æther, and therefore the refrangibility. On the other side the effect is the reverse\*. It would be difficult to exaggerate the import-

\* See Sellmeier, Pogg. *Ann.* vol. cxliii. p. 272.

ance of these facts from the point of view of theoretical optics ; but it lies beside the object of the present paper to go further into the question here.

That a sufficient opacity is as competent as a high optical density to produce an abundant reflection is evident without any analysis. So long as the medium into which the light seeks to penetrate remains nearly at rest, the greater part of the motion must be thrown back without any regard to the *cause* of the approximate quiescence. Whether the sluggishness be due to a great inertia or a correspondingly great friction is in this respect of no importance. In order, however, to account for the reflection from silver (90 or 95 per cent.) without opacity, a very high optical density would be required, much higher than we have any reason to think at all likely. On the other hand, we know that the opacity of metals to light is very great.

In this connexion it is interesting to note that some, and probably many, non-metallic substances possess a quasi-metallic reflecting-power for dark radiation. De la Provostaye and Desains long ago remarked on the large percentage of dark heat reflected from glass, which was much in excess of that calculated from Fresnel's formulæ with the known refractive index. The observation seems to have remained uninterpreted ; but we cannot well be wrong in attributing the extra reflection to an opacity to the rays of dark heat, which, always great, rises somewhere in the spectrum to such a magnitude as to damp the entering rays within a few wave-lengths of the surface. Nothing but direct experiment can inform us what substances are sufficiently opaque to exercise an abnormal reflection ; for the stoppage of radiant heat by a plate of ordinary thickness may well be complete to sense, and yet not sufficiently sudden to give any material assistance in reflection. I am glad therefore to be able to refer to the experiments of the late Professor Magnus (Poggendorff's *Annalen*, vol. cxxxix.), in which he investigates the proportion of heat reflected by plates of various substances, the incident radiation being derived from moderately heated plates of the same or of a different material.

First let us see what fraction of the incident radiation (unpolarized) would be reflected from the surface of a substance having a refractive index of 1·5—about that of glass. If  $\theta$  be the angle of incidence, and  $I$  the corresponding fraction, I find by calculation from Fresnel's formulæ the following :—

$\theta=0$ $I=.040$	$\theta=33^\circ$ $I=.042$	$\theta=45^\circ$ $I=.050$	$\theta=62^\circ$ $I=.100$
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This is for one surface. If the plate be quite transparent, the reflection may be nearly the double of the above. Now for glass at an angle of  $45^\circ$ , Magnus found no smaller value of  $I$  than  $\cdot 084$ ; and as this must be attributed almost, if not entirely, to the first surface, it is clear that something not taken account of in Fresnel's theory must have come into operation. But by far the most remarkable result was with fluor-spar for the reflecting, and rock-salt for the radiating plate. The reflection at  $33^\circ$  was no less than  $\cdot 23$ , at  $45^\circ \cdot 242$ , and at  $62^\circ \cdot 335$ ; and to this the second surface cannot contribute sensibly. Unquestionably therefore the reflecting-power of fluor-spar for a certain kind of dark radiation is greatly in excess of what can be accounted for without an extreme opacity—a result which is the more remarkable because for dark radiation in general fluor-spar is one of the most transparent things known. The reflection from a plate of rock-salt was found to be much the same as from glass; but here, I presume, we may consider both surfaces to be operative, in which case the result is normal\*. It is curious that opacity first diminishes the reflection from a plate, and then when extreme increases it again, and that without limit.

The effect of opacity is represented mathematically by the introduction into the differential equation of a term proportional to the velocity. If we suppose that  $x=0$  is the surface of separation, and that the vibrations are parallel to  $z$  and perpendicular to the plane of incidence  $xy$ , the differential equation in the opaque medium is

$$D_1 \frac{d^2 \zeta_1}{dt^2} + h \frac{d \zeta_1}{dt} = n \left( \frac{d^2 \zeta_1}{dx^2} + \frac{d^2 \zeta_1}{dy^2} \right),$$

where  $h$  is a *positive* constant depending on the opacity, and  $D$  denotes the optical density. In the upper medium  $h=0$ , and the equation is

$$\frac{d^2 \zeta}{dt^2} = \frac{n}{D} \left( \frac{d^2 \zeta}{dx^2} + \frac{d^2 \zeta}{dy^2} \right).$$

Both  $h$  and  $D_1$  are subject to extensive chromatic variations; and the equations are therefore not to be regarded as general equations of motion applicable to all possible cases. It is probable, however, that they represent with sufficient, if not absolute accuracy the laws of motion for a system of plane waves of given period, provided that suitable values of  $h$  and  $D_1$  are assumed. The boundary conditions, according to Green and Cauchy (Phil. Mag. August 1871), expressing that there is no discontinuity

\* According to the experiments of Masson and Jamin, the transmission of a perfectly transparent plate is always about 92 per cent., whether the material be glass, rock-salt, or alum. This is in agreement with the calculation in the text, as about 8 per cent. would be reflected.



in the displacement or strain, are

$$\zeta = \zeta_1, \quad \frac{d\zeta}{dx} = \frac{d\zeta_1}{dx},$$

when  $x=0$ . The system of waves is given by

$$\zeta = \zeta' e^{i(ax+by+ct)} + \zeta'' e^{i(-ax+by+ct)},$$

$$\zeta_1 = \zeta_1 e^{i(a_1x+by+ct)},$$

where

$$a = \frac{2\pi}{\lambda} \cos \theta, \quad b = \frac{2\pi}{\lambda} \sin \theta, \quad c = \frac{2\pi V}{\lambda} = \frac{2\pi}{\tau}.$$

The coefficients  $b, c$  are necessarily the same all through; the multipliers  $\zeta', \zeta'', \zeta_1$  are complex. The boundary conditions being the same as for transparent media, we have (Phil. Mag.

August 1871)  $\frac{\zeta''}{\zeta'} = \frac{a-a_1}{a+a_1}$ , only that now  $a_1$  is not real. In fact

if  $\frac{n}{D} = \gamma$ ,  $\frac{n}{D_1} = \gamma_1$ , we obtain from the differential equations

$$\left. \begin{aligned} c^2 &= \gamma^2 (a^2 + b^2), \\ c^2 - i \frac{h}{D} c &= \gamma_1^2 (a_1^2 + b^2), \end{aligned} \right\}$$

whence

$$\frac{a_1^2 + b^2}{a^2 + b^2} = \frac{\gamma^2}{\gamma_1^2} \left( 1 - i \frac{h}{D_1 c} \right) = \mu^2 \text{ (say).}$$

We see that  $a_1$  is determined as a function of  $a$  and  $b$  by an equation of precisely the same form as for transparent media, the only difference being that  $\mu^2$  is now no longer real. If we suppose  $\theta_1$  to be defined by the equation

$$\sin \theta_1 = \frac{1}{\mu} \sin \theta,$$

we may use the forms previously investigated.

From the physical interpretation of  $\mu^2$ , we see that its real part is positive, and imaginary part negative. If we write

$\mu^2 = R^2 e^{2ix}$ ,  $2\alpha$  must lie between 0 and  $-\frac{\pi}{2}$ . This remark will

be found to be of great importance. For instance, the assumption by Cauchy and others of a real negative value of  $\mu^2$  in their treatment of the so-called longitudinal waves produced when light vibrating in the plane of incidence is reflected from the surface of transparent matter, really corresponds to an unstable medium in which the forces resulting from a displacement tend still further to increase it.

The value of  $\mu$  is  $\text{Re}^{i\alpha}$ , and at perpendicular incidence ( $b=0$ ),

$$\frac{a_i}{a} = \pm \mu, \text{ say } +\mu,$$

as the sign of  $\mu$  is in our power. Now, since the refracted wave is  $\psi_i e^{i(a_i x + b_i y + c_i t)}$ , wherein  $x$  is negative, we see that the real part of  $a_i$  must be positive, while the imaginary part is negative. The same is true of  $\text{Re}^{i\alpha}$ ; or, as  $R$  is taken positive,  $\alpha$  lies between 0 and  $-\frac{\pi}{2}$ . Since, as we have seen,  $2\alpha$  is situated in

the same quadrant,  $\alpha$  must lie between 0 and  $-\frac{\pi}{4}$ . The value

of  $\alpha$  is determined by  $\tan 2\alpha = -\frac{h}{D_i c}$ . It vanishes with  $h$ , and, on the other hand, when  $h : cD_i$  is very great, approximates to  $-\frac{\pi}{4}$ . In this extreme case the real and imaginary parts of  $\mu$  are numerically equal; the imaginary part is never the greater\*.

I have been thus particular to examine the limits between which  $\alpha$  may lie, because it appears to me that there is on this point a serious omission, not to say error, in Eisenlohr's paper. In that investigation the necessity of a limitation on the magnitudes of the real and imaginary parts of  $\mu$  does not appear, mainly because the author has assumed at starting expressions for the incident, reflected, and refracted waves without reference to the differential equations tacitly implied. To suppose, as he does for silver, that  $\alpha=83^\circ$ , and therefore  $2\alpha=166^\circ$ , is tantamount to the assumption of a medium essentially unstable†.

We may now proceed to transform the expression for the reflected wave

$$\frac{\xi''}{\xi'} = \frac{a - a_i}{a + a_i}.$$

In terms of  $\theta$ ,  $\frac{a'}{a} = \frac{\tan \theta}{\tan \theta_i}$ , where  $\sin \theta_i = \frac{1}{\mu} \sin \theta$ . To simplify the expressions, it is convenient, following Cauchy and Eisenlohr,

\* I apprehend that this conclusion is not limited to the particular form of the differential equation which has been assumed. Whatever that may be,  $\mu^2$  will always consist of a real and an imaginary part, of which the former cannot be supposed negative without compromising the stability of the medium.

† In Eisenlohr's paper the incident wave travels in the direction of the positive  $x$ , while I here suppose the opposite. The change amounts to a reversal of the sign of  $c$ ; and thus, on Eisenlohr's supposition, the real part of  $\mu^2$  ought to be positive and the imaginary part also positive; his result requires that the real part should be negative.

to introduce two auxiliary variables, defined by the equation

$$c\epsilon^{iu} = \cos \theta_i = \sqrt{1 - \frac{\sin^2 \theta}{\mu^2}},$$

whence

$$\left. \begin{aligned} c^2 \cos 2u &= 1 - \frac{\cos 2\alpha \sin^2 \theta}{R^2}, \\ c^2 \sin 2u &= \frac{\sin 2\alpha \sin^2 \theta}{R^2}. \end{aligned} \right\}$$

Thus

$$\frac{a_i}{a} = \frac{\sin \theta}{\cos \theta} \cdot \frac{\mu c \epsilon^{iu}}{\sin \theta} = \frac{R c \epsilon^{i(u+\alpha)}}{\cos \theta},$$

$$\frac{\xi''}{\xi'} = \frac{\cos \theta - R c \epsilon^{i(u+\alpha)}}{\cos \theta + R c \epsilon^{i(u+\alpha)}}.$$

If this quantity be called P, and that obtained from it by changing the sign  $i$ , Q,

$$\frac{\xi''}{\xi'} = \sqrt{PQ} \cdot \epsilon^{id},$$

where

$$\tan d = \frac{P - Q}{i(P + Q)}.$$

The intensity of the reflected light is therefore

$$PQ = \frac{\cos^2 \theta - 2Rc \cos \theta \cos(u + \alpha) + R^2 c^2}{\cos^2 \theta + 2Rc \cos \theta \cos(u + \alpha) + R^2 c^2}$$

$$= 1 - \frac{2 \frac{Rc \cos(u + \alpha)}{\cos \theta}}{1 + \frac{R^2 c^2}{\cos^2 \theta}} \div 1 + \frac{2 \frac{Rc \cos(u + \alpha)}{\cos \theta}}{1 + \frac{R^2 c^2}{\cos^2 \theta}} = \tan\left(f - \frac{\pi}{4}\right),$$

if

$$\cot f = \cos(u + \alpha) \frac{2 \frac{Rc}{\cos \theta}}{1 + \frac{R^2 c^2}{\cos^2 \theta}}$$

$$= \cos(u + \alpha) \sin 2 \tan^{-1} \left( \frac{\cos \theta}{Rc} \right).$$

$d$  represents the change of phase; its tangent is given by

$$\tan d = \sin(\alpha + u) \tan 2 \tan^{-1} \left( \frac{\cos \theta}{Rc} \right).$$



These are Cauchy's formulæ for light polarized in the plane of incidence. At perpendicular incidence,

$$\theta=0, \quad \theta_i=0, \quad c=1, \quad u=0,$$

whence

$$\tan f = \frac{1+R^2}{2R \cos \alpha} = \frac{1}{2 \cos \alpha} \left( R + \frac{1}{R} \right),$$

which may be expressed in terms of  $\gamma_i$  and  $h$  by means of:—

$$R^2 \cos 2\alpha = \frac{\gamma^2}{\gamma_i^2}; \quad R^2 \sin 2\alpha = -\frac{\gamma^2}{\gamma_i^2} \frac{h}{cD_i}.$$

In the case of metal the reflected light is a large percentage, and therefore  $\tan f$  is considerable. This can only be when  $R$  is great; and then  $\frac{1}{R}$  is relatively very small. Approximately therefore

$$\tan f = \frac{R}{2 \cos \alpha} = \frac{\gamma}{\sqrt{2} \cdot \gamma_i} \frac{\sqrt{1 + \frac{h^2}{c^2 D_i^2}}}{\sqrt{1 + \sqrt{1 + \frac{h^2}{c^2 D_i^2}}}};$$

or, if  $\frac{h}{cD_i}$  is considerable,

$$\tan f = \frac{\gamma}{\sqrt{2} \cdot \gamma_i} \sqrt{1 + \frac{h^2}{c^2 D_i^2}} = \frac{\gamma}{\gamma_i} \sqrt{\frac{h}{2cD_i}} \text{ nearly.}$$

Also

$$\frac{\gamma}{\gamma_i} = \sqrt{\frac{D_i}{D}};$$

and thus, when the opacity is so great that the reflected light is a large fraction of the whole, its intensity will be

$$\frac{\sqrt{h} - \sqrt{2Dc}}{\sqrt{h} + \sqrt{2Dc}}.$$

If we suppose that  $h$  is constant for the different waves of light, we find that the reflection is better and better the longer the wave, since  $c$  varies inversely as the period of vibration. Most metals, it would appear, reflect the red rays in greater quantity than the more refrangible, and dark heat better than any.

The wave entering the metal is represented by  $\zeta_i e^{i(a_i x + ct)}$ , or, on substitution of its value for  $a_i$ ,

$$\zeta_i e^{-R \sin \alpha x} \cdot e^{i(R \cos \alpha x + ct)}.$$

The velocity of wave-propagation is  $\frac{c}{R \cos \alpha}$  against  $\frac{c}{a}$  in air.

Thus in a certain sense  $R \cos \alpha$  (that is, the *real part* of  $\mu$ ) may be regarded as the refractive index of the metal for the kind of light under consideration; but I wish to remark that great confusion has arisen in the use of the expression "refractive index" as applied to metallic or quasi-metallic bodies, the same name being given to quantities which, though coincident for transparent matter, may here differ widely.

Expressed in terms of  $\gamma$ , and  $h$ ,

$$R \cos \alpha = \frac{\gamma}{\gamma_1} \sqrt{\frac{1 + \sqrt{1 + \frac{h^2}{cD_1^2}}}{2}};$$

or if  $\frac{h}{cD_1}$  be very large,

$$R \cos \alpha = \frac{\gamma}{\gamma_1} \sqrt{\frac{h}{2cD_1}} = \sqrt{\frac{h}{2Dc}},$$

which, we have seen, presumably increases with the period of vibration. In this approximation we have supposed that the influence of opacity is paramount, so that  $\sin 2\alpha = -1$ , and

$$R \cos \alpha = -R \sin \alpha = \frac{R}{\sqrt{2}}.$$

The wave-length within the medium may be taken to be

$$\lambda' = \frac{\lambda}{R \cos \alpha} = \sqrt{\frac{2Dc}{h}} \cdot \lambda = 2 \sqrt{\frac{\pi VD}{h}} \cdot \lambda^{\frac{1}{2}} \text{ approx.,}$$

on substitution of the value of  $c$ . Hence, if  $h$  be constant, the wave-length in the metal varies as the square root of the wave-length in air. The quantity here called the internal wave-length is that which physically best deserves the name; and it is connected with what we have called the refractive index by the usual relation,

Internal wave-length = external wave-length  $\div$  refractive index; but it must be remembered that, from an analytical point of view the internal wave-length and refractive index are imaginary, being denoted by  $\lambda \div \mu$  and  $\mu$  respectively.

The factor expressing the absorption is

$$e^{-R \sin \alpha x} = e^{-\frac{2\pi x}{\lambda} R \sin \alpha},$$

or, in terms of  $\lambda'$ ,

$$e^{-\frac{2\pi x}{\lambda'} \tan \alpha},$$

where, it will be remembered, both  $x$  and  $\tan \alpha$  are negative, showing that, if  $\alpha$  be constant, the penetration expressed in terms

of  $\lambda'$  is always the same. In cases where the influence of opacity is overwhelming,  $\tan \alpha = -1$ .

In order to form an idea of the sort of magnitudes with which we are dealing, let us take silver—an extreme case. Exact measurements of the percentage of light reflected at perpendicular incidence are wanting (so far as I know); but De la Provostaye and Desains found in some cases a reflection of dark heat amounting to 95 per cent. Using this in our formulæ, we find

$$R + \frac{1}{R} = 2 \cos \alpha \cdot 39.$$

Now, since  $\cos \alpha$  can never be less than  $\frac{1}{\sqrt{2}}$ , it follows that  $\frac{1}{R}$  can be neglected in comparison with  $R$ ; and thus  $R = 80 \cos \alpha$ ;

$$R \cos \alpha = 80 \cos^2 \alpha; \quad R \sin \alpha = 40 \sin 2\alpha.$$

If we further suppose that the great value of  $R$  is due to opacity, we may put  $\cos \alpha = \frac{1}{\sqrt{2}}$ , and

$$R = 40 \sqrt{2}, \quad R \cos \alpha = 40, \quad R \sin \alpha = -40.$$

Thus  $\lambda' = \frac{\lambda}{40}$ ; otherwise the ratio of  $\lambda' : \lambda$  is still smaller.

For the metals it is probable that of the total reflection the greater part is due to opacity; in other cases it often happens that the effect of opacity is only a slight increase of the reflection that would otherwise take place. Let us inquire what the strength of absorption must be.

If  $\mu_0$  denote the refractive index which the medium would possess in virtue of its density alone  $\left(\frac{\gamma}{\gamma_1}\right)$ , we have

$$R^2 \cos 2\alpha = \mu_0^2, \quad R^2 \sin 2\alpha = -\mu_0^2 \frac{h}{D_1 c};$$

while the reflection is given by

$$\text{Reflection} = \frac{\tan f - 1}{\tan f + 1}$$

and

$$\tan f = \frac{1}{2 \cos \alpha} \left( R + \frac{1}{R} \right) = \frac{1}{2 \cos \alpha} \left( \frac{\mu_0}{\sqrt{\cos 2\alpha}} + \frac{\sqrt{\cos 2\alpha}}{\mu_0} \right);$$

from which it appears that, when  $\alpha$  is small, its effect depends on  $\alpha^2$ .

On the other hand the factor representing the absorption is

$$e^{-\frac{2\pi x}{\lambda} R \sin \alpha}, \text{ or approximately } e^{-\frac{2\pi x}{\lambda} \mu_0 \alpha^2},$$



in which the coefficient of  $x$  varies as  $\alpha$ . For instance, let  $\alpha^2 = \frac{1}{100}$ . The effect on reflection would be insensible to ordinary observation, though the opacity is so great as to halve the light within a distance equal to the wave-length in air. Thus it is evident that, in order to aid in reflection, opacity must be very extreme.

We have hitherto supposed that the reflection takes place at the bounding surface of the opaque medium and air; but it is easy to adapt our formulæ so as to express the result when the first medium, still supposed transparent, or at least not very opaque, has a refractive index  $\mu'$  different from unity. The only change required is to write  $R \div \mu'$  for  $R$ . Thus at perpendicular incidence,

$$\tan f = \frac{2}{\cos \alpha} \left( \frac{R}{\mu'} + \frac{\mu'}{R} \right).$$

If the reflection be still so good as to allow of the neglect of the second term, we have

$$\tan f = \frac{2R}{\mu' \cos \alpha}.$$

The reflection when light strikes from glass on silver would be considerably less perfect than when the first medium is air; in fact the percentage not reflected

$$= \frac{2}{1 + \tan f} = \frac{\mu' \cos \alpha}{R} \text{ approx.}$$

So much for vibrations perpendicular to the plane of incidence. When we pass to the consideration of vibrations in that plane, we are embarrassed by difficulties, of the kind met with in the theory of ordinary reflection, here presenting themselves in an aggravated form. If, following Green, we assumed the equations of motion applicable to elastic solids with the addition of terms proportional to the velocity to represent the frictional loss, and further supposed that the rigidity is the same in the two media, while the compressibility is indefinitely small, we should arrive at results differing only from his by the substitution of an imaginary for a real refractive index. But we know from experiment that Green's results are not verified for transparent media without a modification of doubtful significance, and of magnitude *increasing rapidly with  $\mu$* . It is therefore useless to attempt to apply Green's results. The only other course appears to be to start from Fresnel's tangent-formula, and transform that, as we have done the one involving sines, by the introduction of a complex refractive index (and angle of refraction). This is what has been done by Cauchy and Eisenlohr. Following a process

similar to that used for vibrations normal to the plane of incidence and with the same notation, we find that the intensity of the reflected light is represented by  $\tan\left(g - \frac{\pi}{4}\right)$ , where

$$\cot g = \cos(\alpha - u) \sin 2 \tan^{-1}\left(\frac{c}{R \cos \theta}\right),$$

while the change of phase  $d'$  is given by

$$\tan d' = \sin(\alpha - u) \tan 2 \tan^{-1}\left(\frac{c}{R \cos \theta}\right).$$

However, what we should most require for comparison with experiment relates to the *relative* intensities and changes of phase of the two polarized components; and these are directly obtained by Eisenlohr by transforming Fresnel's corresponding expression\* after the introduction of the complex refractive index. If the ratio of the *amplitudes* be called  $\tan \beta$ , we have

$$\cos 2\beta = \cos(\alpha + u) \sin 2 \tan^{-1}\left(\frac{\sin^2 \theta}{cR \cos \theta}\right),$$

$$\tan(d' - d) = \sin(\alpha + u) \tan 2 \tan^{-1}\left(\frac{\sin^2 \theta}{cR \cos \theta}\right),$$

$c$  and  $u$  being determined as before. Eisenlohr has compared these formulæ with measurements made by Jamin relating to the so-called principal angle of incidence (making  $d' - d$  equal to  $\frac{\pi}{4}$ ) and the corresponding ratio of amplitudes, and has deduced, as I have already remarked, values of the constants which make the real part of  $\mu^2$  negative, and are therefore inadmissible. Another argument leading to the same conclusion is as follows.

Consider a case in which  $\mu^2$  is so considerable that  $c$  is sensibly equal to unity and  $\alpha$  to zero, or, in other words, so refractive that the entering ray is always sensibly parallel to the normal of the surface; and let the incident ray strike the surface at that particular angle which gives a relative phase-difference of a quarter of a period. The angle in question is that determined by

$$\frac{\sin^2 \theta}{cR \cos \theta} = 1;$$

so that

$$\cos 2\beta = \cos \alpha.$$

Since  $\alpha$  must lie between 0 and  $-\frac{\pi}{4}$ ,  $\cos 2\beta$  must be comprised

\*  $\frac{\cos(\theta, +\theta)}{\cos(\theta, -\theta)}.$

between the limits 1 and  $\frac{1}{\sqrt{2}}$ . Now in order that the reflection may be perfect at all angles of incidence, it is only necessary that the density or opacity or both should be sufficiently large; and then  $\cos 2\beta$  must be sensibly equal to zero. And yet if the formulæ under consideration were true, there would always be a certain angle of incidence making the ratio of the two polarized components very different from unity—a state of things incompatible with a nearly complete reflection. I do not think that the failure of the formulæ for light vibrating in the plane of incidence need cause surprise, when it is considered that Fresnel's tangent-formula, which forms the starting-point of the investigation, is not verified even with transparent media, and differs more and more from the truth as the refracting-power increases. The failure of theory is the more unfortunate, because the relative change of phase and ratio of amplitudes for the two polarized components are precisely the quantities best adapted to experimental measurement. As it is, we must conclude, I fear, that the careful investigations of Jamin on the subject are at present unavailable for the purpose of forming an estimate of the values of the density and opacity of the various metals. Experiments on the absolute reflecting-power of the metals for the different parts of the spectrum at perpendicular incidence would be valuable and probably easy; but they do not appear to have been attempted.

It has hitherto been supposed that there is no interruption in the continuity of the metallic medium within such a distance from the surface that the intromitted wave is still sensible. This is a very different thing from assuming, as it has been asserted that the theory does\*, that the reflection takes place entirely from the surface, if indeed such an assumption could have any meaning. When, as in the experiments of Quincke, the metallic layer is so thin as to transmit a sensible quantity of light, it is clear that the theory requires modification. If the medium on the two sides of the metal are optically similar, a sufficient reduction of the thickness of the layer must at last result in a destruction of the reflection, just as with thin transparent plates.

One of the most remarkable of Quincke's results relates to the influence of a thin metallic layer on the *phase* of the transmitted light. In many cases the phase was accelerated so as to be in advance of what would correspond to a layer of air in place of the silver—an effect which, according to ordinary ideas, would imply a refractive index less than unity. However, it is not difficult to see that, in regard to the effect on the phase of the

\* Wüllner, *Lehrbuch der Experimentale Physik*, vol. ii. p. 471.



transmitted wave, the influence of opacity is altogether different from that of density. According to a method used by me in the investigation of the light scattered from very small particles (as in the sky)\*, we may suppose that the incident wave passes on undisturbed, if suitable forces are imagined to act on the æther in the metal in order to compensate for the alteration of optical properties. In the case when the thickness of the metallic plate is small compared to the internal wave-length, this point of view possesses considerable advantages, and gives a clear insight into the peculiarities of the question. The forces which we must suppose to act in the region of the metallic layer divide themselves into two groups—one dependent on variation of density, and the other on opacity. The first set correspond in phase with the acceleration of the æther, the second with the velocity. There is thus a difference of a quarter of a period. In my former paper I showed that the effect of a force acting at any point is to produce at another, distant  $r$  from it, a disturbance whose retardation relative to the force is  $r$  simply. In our case each particle of the plate must be considered to be a centre from which a disturbance emanates; and it will readily appear that the phase due to the whole system of forces is just a quarter of a period behind that corresponding to that element of disturbance which suffers least retardation. In fact, if we divide the whole plate into Huyghens's zones, we know that the effect of the whole is the half of that of the first zone. Now the phase of the disturbance due to the first zone is the mean between that corresponding to its centre and circumference, of which the latter is half a period behind the former. Thus the wave produced by the set of forces due to the alteration of density is a quarter of a period behind the direct wave which has been supposed to pass through undisturbed. The effect of the disturbance is accordingly a maximum on the phase (calculated without allowance for the metallic plate), and a minimum (to the order of approximation here considered, vanishing) on the intensity. It is just the opposite with the second group of forces due to the opacity, which are originally a quarter of a period behind the first. The disturbance due to them will be half a period behind the direct wave with which it has to be compounded, and therefore produces a maximum effect on the intensity and a vanishing one on the phase. A very thin film can produce no effect on the phase of the transmitted light in virtue of its opacity, however great, but acts just as if it were deprived of its absorbent power and reduced to the condition of an ordinary transparent plate. Of course this does not explain the acceleration of phase found by Quincke; but it shows at least that

\* Phil. Mag. February 1871.

we must be prepared to distinguish between the effects of density and opacity, though these are in the same direction so far as regards the magnitude of the reflection &c.

Let us then consider analytically the behaviour of a thin metallic plate when light is incident normally upon it. Above let the disturbance be

$$\zeta = \epsilon^{i(ax+ct)} + B_1 \epsilon^{i(-ax+ct)};$$

below the plate,

$$\zeta = A_2 \epsilon^{i(ax+ct)}.$$

In the interior we must introduce both kinds of exponentials, in order to represent the reflection from the second surface. Thus

$$\zeta' = A'_1 \epsilon^{i(a_1x+ct)} + B'_1 \epsilon^{i(-a_1x+ct)},$$

where  $a_1 = \mu a$  as before.

The conditions to be satisfied are the continuity of  $\zeta$  and  $\frac{d\zeta}{dx}$  at the two surfaces of separation, viz. when  $x=0$  and when  $x=-\delta$ , which give four simple equations for the determination of  $B_1, A'_1, B'_1, A_2$ . On elimination of  $A'_1, B'_1$ , we obtain

$$B_1 = - \frac{(\mu^2 - 1) i \sin \mu a \delta}{2\mu \cos \mu a \delta + (\mu^2 + 1) i \sin \mu a \delta},$$

$$A_2 = \frac{\epsilon^{ia\delta}}{\cos \mu a \delta + \left(\frac{\mu^2 + 1}{2\mu}\right) i \sin \mu a \delta}.$$

These expressions contain the ordinary results for transparent plates. Considering  $\mu$  real, the reflected wave is

$$\frac{(\mu^2 - 1) \sin \mu a \delta}{\sqrt{4\mu^2 \cos^2 \mu a \delta + (\mu^2 + 1)^2 \sin^2 \mu a \delta}} \epsilon^{i(-ax+ct+e)},$$

where

$$\tan e = - \frac{\mu^2 + 1}{2\mu} \tan \mu a \delta.$$

Similarly the transmitted wave is

$$\frac{1}{\sqrt{\cos^2 \mu a \delta + \left(\frac{\mu^2 + 1}{2\mu}\right)^2 \sin^2 \mu a \delta}} \epsilon^{i(ax+ct+a\delta+e')},$$

where

$$\tan e' = - \frac{\mu^2 + 1}{2\mu} \tan \mu a \delta.$$

If  $\mu a \delta$  be very small, the expression for the wave becomes approximately

$$\epsilon^{i(ax+ct+a\delta - \frac{\mu^2 + 1}{2} a \delta)} = \epsilon^{i(ax+ct - a \delta \frac{\mu^2 - 1}{2})}.$$

this case there is no loss of intensity in the transmitted light, and the retardation is  $\frac{\mu^2 - 1}{2}\delta$ .

But if  $\mu$  be complex (equal to  $R\epsilon^{ia}$ ),

$$B_1 = \frac{-(R^2\epsilon^{2ia} - 1)i \sin(R\epsilon^{ia}a\delta)}{2R\epsilon^{ia} \cos(R\epsilon^{ia}a\delta) + i(R^2\epsilon^{2ia} + 1) \sin(R\epsilon^{ia}a\delta)}.$$

The intensity of the reflected light is to be found by multiplying  $B_1$  by the quantity derived from it by changing the sign of  $i$ . The numerator of the resulting fraction is

$$(R^4 - 2R^2 \cos 2\alpha + 1) \sin(R\epsilon^{ia}a\delta) \sin(R\epsilon^{-ia}a\delta).$$

The product of sines is the half of

$$\begin{aligned} & \cos\{2R \sin \alpha \cdot ia\delta\} - \cos\{2R \cos \alpha \cdot a\delta\} \\ &= \frac{1}{2} \{e^{2R \sin \alpha \cdot a\delta} + e^{-R \sin \alpha \cdot a\delta}\} - \cos\{2R \cos \alpha \cdot a\delta\}. \end{aligned}$$

We may infer that the intensity of the reflected light is nearly proportional to

$$1 - \frac{2 \cos\{2R \cos \alpha \cdot a\delta\}}{e^{2R \sin \alpha \cdot a\delta} + e^{-2R \sin \alpha \cdot a\delta}}.$$

For transparent media the sum of exponentials reduces to the constant 2, but for opaque media it increases rapidly with  $\delta$ . After the first, corresponding to  $\delta=0$ , the minima are no longer zero, and soon all fluctuation becomes insensible.

Another effect of the exponential terms is to displace the position of the maxima and minima with respect to  $\delta$ . They tend to occur earlier than they otherwise would do. In our ignorance of the values of the constants it seems hardly worth while to follow the result more minutely.

The transformation of  $A_2$  when  $\mu$  is complex leads to a long expression; and I will therefore confine myself to the particular case of a very thin layer, whose thickness does not amount to more than a small fraction of the wave-length.

Let the reduced expression for the transmitted wave be

$$A_2 e^{i(ax+ct)} = (\text{amplitude}) e^{i(ax+a\delta+ct+e')},$$

Then  $e'$  is given by the equation

$$\tan e' = \frac{D' - D}{i(D' + D)},$$

if we denote the denominator of the expression for  $A_2$  by  $D$ , that derived from it by changing the sign of  $i$  by  $D'$ . Now



$$D = 1 - \frac{R^2 \epsilon^{2ia} (a\delta)^2}{2} + \frac{1 + R^2 \epsilon^{2ia}}{2} ia\delta + \text{cubes in } \delta,$$

$$D' = 1 - \frac{R^2 \epsilon^{-2ia} (a\delta)^2}{2} - \frac{1 + R^2 \epsilon^{-2ia}}{2} ia\delta + \text{cubes in } \delta;$$

$$\therefore D' - D = R^2 a^2 \delta^2 i \sin 2\alpha - ia\delta(1 + R^2 \cos 2\alpha),$$

$$D' + D = 2 - R^2 a^2 \delta^2 \cos 2\alpha - R^2 \sin 2\alpha \cdot a\delta;$$

$$\therefore \tan e' = \frac{-a\delta(1 + R^2 \cos 2\alpha) + R^2 \sin 2\alpha \cdot a^2 \delta^2}{2 - R^2 \cos 2\alpha \cdot a^2 \delta^2 - R^2 \sin 2\alpha \cdot a\delta}$$

$$= -\frac{a\delta(1 + R^2 \cos 2\alpha)}{2} \left\{ 1 + \frac{R^2 \sin 2\alpha \cdot a\delta}{2} \left( \frac{1}{2} - \frac{1}{1 + R^2 \cos 2\alpha} \right) \right\}.$$

As a first approximation,

$$e' = -\frac{a\delta}{2} (1 + R^2 \cos 2\alpha);$$

and the transmitted wave is

$$(\text{amplitude}) e^{i\left\{a\left(x - \frac{R^2 \cos 2\alpha - 1}{2} \delta\right) + ct\right\}};$$

so that the retardation is  $\frac{R^2 \cos 2\alpha - 1}{2} \delta$ , independent of the opacity, as we have already seen it ought to be.

The second term in the approximate value of  $e'$  has a contrary effect to the first, because  $\sin 2\alpha$  is negative. Moreover  $\sin 2\alpha$  is numerically large. This may account for the acceleration of phase observed by Quincke—though if this explanation be correct, there must always be a retardation when the film is thin enough. It may happen that, in virtue of the great opacity of silver, its elimination by a reduction of the thickness may be impracticable without at the same time bringing the retardation due to density below the point at which it could be detected.

Terling Place, Witham,  
January 18, 1872.

*Postscript.*—Since the above was written there has appeared a paper by O. E. Meyer, entitled "An Attempt to account for Anomalous Dispersion of Light"\*, in which the author arrives at an expression for the refractive index equivalent to that found above as the value of  $R \cos \alpha$ , namely

$$R \cos \alpha = \frac{\gamma}{\gamma_1} \sqrt{\frac{1 + \sqrt{1 + \frac{h^2}{cD_1^2}}}{2}}.$$

Considering  $h$  constant, he sees in this an explanation of anoma-

\* Pogg. Ann. vol. cxlv. p. 80, translated in Phil. Mag. vol. xliii. p. 295.  
Phil. Mag. S. 4, Vol. 43, No. 287, May 1872. Z

lous dispersion, inasmuch as  $R \cos \alpha$  increases with  $\lambda (c \propto \lambda^{-1})$ . In this view I cannot at all agree. Meyer seems to have overlooked the fact that  $h$  (in his notation  $\kappa$ ), the constant of opacity, is subject to enormous chromatic variations, in comparison with which those of  $\lambda$  may be treated as quite insignificant. But this is not all. It has been laid down by Kundt as the result of his observations—and a very remarkable and suggestive result it is—that the anomalous effect is not confined to those rays which are intensely absorbed. Probably indeed the effect vanishes at that part of the spectrum which corresponds to the centre of the absorption-band, where, according to Meyer's formula (though not his interpretation of it), it should be a maximum. I have already indicated what appears to me to be the true mechanical character of the phenomenon by an illustration derived from ordinary dynamics. The mathematical analysis of the problem referred to, which turns up in almost all branches of physics dealing with vibrations, is well known and therefore need not be given here.

April 5.

XLI. *Reply to Professor Clausius.* By P. G. TAIT\*.

WHEN Professor Clausius succeeds in making his own countrymen regard him as the discoverer of the Dissipation of Energy (see, for instance, Helmholtz, *Populäre wissenschaftliche Vorträge*, Heft ii. p. 117) it will be time enough to complain that foreigners do not give him that credit.

As regards the question to whom is due the credit of first correctly adapting Carnot's magnificently original methods to the true theory of heat, it is only necessary to compare the *Axiom* of Professor Clausius's first paper (the only one which has a chance of priority over Thomson) with the behaviour of a thermo-electric circuit in which the hot junction is at a temperature higher than the neutral point, and where therefore heat *does, of itself, pass from a colder to a hotter body*. A thermo-electric battery, worked with ice and boiling water, is capable of raising to incandescence a fine wire, giving another excellent instance of the fallacy of the so-called axiom.

Professor Clausius has rendered many services to science, especially in the Kinetic Theory of Gases; but he has done, and seems to take credit in doing, uncompensated mischief by his introduction of what he calls *innere Arbeit* and *Disgregation*. In our present ignorance of the nature of matter, such ideas can do only harm; and no one will dispute his full claim to originality as regards *them*.

\* Communicated by the Author.

XLII. *On Hamilton's Principle and the Second Proposition of the Mechanical Theory of Heat.* By C. SZILY\*.

THE history of the development of modern physics speaks decidedly in favour of the view that only those theories which are based on mechanical principles are capable of affording a satisfactory explanation of the phenomena.

The first proposition of the theory of heat would certainly not have spread so quickly, and in a year or two have penetrated every branch of physical science, if it had not been preceded by an analogous proposition, viz. that of the equivalence of work and *vis viva*. The perfect concordance which prevails between the first proposition of the theory of heat and a fundamental principle of mechanics secured to both the possibility of quickly penetrating all branches of physics, although up to the present time the mechanical equivalent of light, of chemical affinity, and of electricity are not yet known.

The second proposition of the theory of heat is scarcely two years more recent than the first; its bearing is already not less—nay, when we take into consideration the defective development of the other branches of physics, it will perhaps be still greater than that of the first; and yet, while the first proposition, one might say, took the whole domain of physical science by storm, the second has hitherto scarcely been able to extend beyond the limits of the theory of heat.

Wherein lies the reason of this striking phenomenon? It appears to me, partly in this—that the second proposition of the theory of heat did not find in mechanics any correlative principle so generally known as the first did in the principle of the equivalence of work and *vis viva*. For, if we express the second proposition in words or in mathematical symbols, we can hardly say that it reminds us of any principle whatever in mechanics.

Although the analogous proposition of mechanics was not recognized, or at least not placed in apposition with that of the theory of heat, yet almost every one had no doubt that there must exist in dynamics a relation similar to this second proposition; for, if heat is only a particular kind of motion, that equation of the theory of heat must be contained in the equations relative to the most universal motion. The only question was, *Which equation in dynamics leads, in a certain special case, to the second proposition of the theory of heat?* or, inversely, *To which equation in dynamics can the second proposition of the theory of heat be reduced?*

\* Communicated by the Author. From the *Magyar Akadémia Értekezései* (Proceedings of the Hungarian Academy of Science), having been read December 11, 1871.



The first who, to my knowledge, occupied himself with this question was Ludwig Boltzmann. His memoir relative to it was presented to the Vienna Academy on the 8th of February, 1866, and appeared in the *Sitzungsberichte* under the title "On the Mechanical Signification of the Second Proposition of the Theory of Heat."

Independently of Boltzmann, and evidently unaware of the existence of his memoir, Clausius laid before the Society for Natural and Medical Science of the Lower Rhine, on the 7th November 1870, a memoir entitled "On the Reduction of the Second Axiom of the Mechanical Theory of Heat to general Mechanical Principles"\*.

The result in both memoirs is much the same:—"The second proposition of the mechanical theory of heat is capable of being explained from the principles of analytical mechanics. *For this purpose, however, new and peculiar developments are necessary*; and the calculations relative to it are very similar to those generally made use of in order to demonstrate the so-called 'principle of least action.' "

Clausius investigated, in the first place, what connexion exists between the periodical motions of a material point in a closed path, presupposing conservative forces (that is, possessing a force-function); he next discusses the possible causes of alteration of path, and shows the validity of the equations advanced in the different cases. In the third part of his memoir Clausius passes from this simple case to more complicated ones, by assuming a whole system of reciprocally acting material points in periodical motion in closed paths. He then generalizes the equations for such stationary motions as do not take place in closed paths. The mechanical equation thus deduced, and with which he next compares the second proposition of the theory of heat, is the following:—

$$\delta L = \delta \bar{T} + 2\bar{T}\delta \log i,$$

in which  $\delta L$  denotes the work which the conservative forces must accomplish that the system may pass from a given stationary motion into another stationary motion;

$$\delta \bar{T} = \delta \frac{1}{i} \int_0^i \sum \frac{mv^2}{2} . dt,$$

signifies the meanwhile resulting variation of the mean *vis viva* of the system, and  $i$  the duration of a motion-period.

My attention having been directed by Clausius's memoir to the relation subsisting between his equation and the principle of

\* Phil. Mag. S. 4. vol. xlii. p. 161.

least action, on the one hand, and the second proposition of the theory of heat, on the other, I considered that it would not be uninteresting to investigate the question, *what connexion subsists between the second proposition of the theory of heat and Hamilton's dynamic principle\* which relates to varying action.*

Hamilton's principle may be expressed as follows†:—

If any conservative system of material points be in any free motion between any initial and final configuration, the following equation will be universally valid for an infinitely small alteration of this motion:—

$$\delta A = \sum m v_1 \delta s_1 - \sum m v_0 \delta s_0 + i \delta E, \quad . \quad . \quad . \quad (1)$$

where  $m$  is the mass of a point of the system;  $\delta s_1$  and  $\delta s_0$  are the displacements of the same point from the previous final configuration into the new one, and from the previous initial configuration into the new one, respectively;  $v_1$  and  $v_0$  denote the velocity, measured always in the direction of displacement, of the same point in the earlier final configuration and initial configuration respectively;  $i$  is the time during which the system passes from the first initial configuration to the first final configuration.  $\delta A$  is the difference of action, and  $\delta E$  the difference of total energy, between the new and the former path. By action is understood the time-integral of twice the *vis viva* for the time during which the system passes from the initial to the final configuration; by total energy, the sum of the kinetic and potential energies present at a determined moment. Thus both  $A$  and  $E$  in one and the same path are constant, in whatever configuration the system may be, but in general variable from path to path.

If the total *vis viva* of the system at a fixed time be  $T$ , then

$$A = \int_0^i 2T \cdot dt. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

If, further,  $U$  be the potential energy of the system at the same instant, then the total energy

$$E = T + U.$$

Both  $T$  and  $U$  have different values at different points of the path; but their sum is the same constant quantity for all points of the path. Hence

$$i \cdot E = \int_0^i (T + U) dt. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Taking into consideration equations (2) and (3), Hamilton's

\* Hamilton: "On a General Method in Dynamics," Phil. Trans. 1834; "Second Essay on a General Method in Dynamics," *ibid.* 1835.

† Conf. Sir W. Thomson and P. G. Tait, 'Treatise on Natural Philosophy,' vol. i. p. 235.

principle, expressed in equation (1), assumes the following more familiar form :—

$$\delta \int_0^i (T - U) dt = \Sigma m v_1 \delta s_1 - \Sigma m v_0 \delta s_0 - E \delta i. \dots$$

Returning to the first form, let us inquire when is the variation of the action independent of the initial and final configurations? This will be the case when

$$\Sigma m v_1 \delta s_1 = \Sigma m v_0 \delta s_0, \text{---} \dots \dots \dots (4)$$

that is, when the action in the time during which the system is passing from the previous to the new initial configuration is just equal to the action during the passage of the system from the previous to the new final configuration.

The condition in (4) is, *e. g.*, satisfied :—

When the paths all start from a common initial position and pass over to a common final position ; for then  $\delta s_1 = 0$  and  $\delta s_0 = 0$  for every point of the system ; or

When the paths are closed and the motions periodic ; for then  $\delta s_1 = \delta s_0$  and  $v_1 = v_0$  for every point of the system ; or

When the paths are not closed, but the displacement of every point in the initial and in the final configuration proceeds according to the equation  $v_1 \delta s_1 = v_0 \delta s_0$ .

All these are only special cases ; the general condition is given in equation (4).

When, in the motion of the system, the alteration of the motion satisfies equation (4), Hamilton's principle can be expressed very simply :—

$$\delta A = i \delta E ; \dots \dots \dots (5)$$

that is, *the variation of the action in the transition from one path to another is equal to the product of the time necessary for the accomplishment of the path, into the variation of the total energy.*

Let  $\bar{T}$  be the mean value of the total *vis viva* during a period of the motion ; then

$$i \bar{T} = \int_0^i T dt,$$

and

$$A = 2i \bar{T}^*,$$

\* Introducing this value of A into equation (5), and remembering that, according to equation (3),

$$E = \bar{T} + \bar{U},$$

it follows that

$$2i \delta \bar{T} + 2\bar{T} \delta i = i \delta \bar{T} + i \delta \bar{U},$$

and hence

$$\delta \bar{U} = \delta \bar{T} + 2\bar{T} \delta \log i ;$$

and this is the equation of Clausius.



whence it follows that

$$\delta E = \bar{T} \cdot \delta \log (iT)^2.$$

Or if we replace the symbol of variation by that of differentiation, and, instead of by  $\bar{T}$ , denote the mean value of the *vis viva* simply by  $T$ , then

$$\frac{dE}{T} = d \log (iT)^2. \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Let us suppose this equation integrated for a circular process, and bear in mind that  $iT$  has the same value at the end of the process as at its beginning; then

$$\int \frac{dE}{T} = 0; \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

and this is the equation which Clausius, in the year 1854, first published as an expression of the second proposition of the theory of heat, for conservative circular processes.

When the system is not conservative, and therefore, besides the forces with a force-function, either the friction of solid bodies, or viscosity of fluids, or other such like dissipative forces operate, the energy lost  $\delta R$  in overcoming the resistance must be added, in the last term of equation (1) expressing Hamilton's principle, to the variation of the total energy, and therefore equation (5) is transformed into the following:—

$$\delta A = i(\delta E + \delta R). \quad . \quad . \quad . \quad . \quad . \quad (8)$$

Taking into account that  $\delta R$  always denotes energy lost, and hence is always positive in this equation, equation (7) changes into the following inequality:—

$$\int \frac{dE}{T} < 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

This is the identical one advanced by Clausius, in the theory of heat, for dissipative circular processes.

Hereby the second proposition of the theory of heat is reduced to a universal principle of dynamics. *What in thermodynamics we call the second proposition, is in dynamics no other than Hamilton's principle, the identical principle which has already found manifold applications in several branches of mathematical physics.*

### XLIII. Note on Recurrent Vision.

By Professor C. A. YOUNG, of Dartmouth College\*.

**I**N the course of some experiments with a new double-plate Holtz machine, belonging to the college, I have come upon a very curious phenomenon, which I do not remember ever to

\* Communicated by the Author from the American Journal of Science and Art for April 1872.

have seen noticed. The machine gives, easily, intense Leyden-jar sparks from 7 to 9 inches in length, and of most dazzling brilliance, at the rate of seventy a minute. When, in a darkened room, the eye is screened from the direct light of the spark, the illumination produced is sufficient to render every thing in the apartment perfectly visible; and, what is remarkable, every conspicuous object is seen *twice* at least, with an interval of a trifle less than a quarter of a second—the first time vividly, the second time faintly; often it is seen a third, and sometimes (but only with great difficulty) even a fourth time. The appearance is precisely as if the object had been suddenly illuminated by a light at first bright, but rapidly fading to extinction, and as if, while the illumination lasted, the observer were winking as fast as possible.

I see it best by setting up in front of the machine, at a distance of 8 or 10 feet, a white screen having upon it a black cross, with arms about 3 feet long and 1 foot wide, made of strips of cambric. That the phenomenon is really subjective, and not due to a succession of sparks, is easily shown by swinging the screen from side to side. The black cross, at all the periods of visibility, occupies the same place, and is apparently stationary. The same is true of a stroboscopic disk in rapid revolution: it is seen several times by each spark, but each time in the same position. There is no apparent multiplication of a moving object of any sort.

The interval between the successive instants of visibility was measured roughly as follows:—A tuning-fork, making  $92\frac{1}{2}$  vibrations per second, was adjusted so as to record its motion upon the smoked surface of a revolving cylinder; and an electromagnet was so arranged as to record any motion of its armature upon the trace of the fork; a key connected with this magnet was in the hands of the observer. An assistant turned the machine slowly, so as to produce a spark once in two or three seconds, while the observer manipulated the key.

In my own case the mean of a dozen experiments gave 0·22 second as the interval between the first and second seeing of the cross upon the screen, the separate results varying from 0·17 to 0·30 second. Another observer found 0·24 second as the result of a similar series.

Whatever the true explanation may turn out to be, the phenomenon at least suggests the idea of a *reflection of the nervous impulse* at the nerve extremities—as if the intense impression upon the retina, after being the first time propagated to the brain, were there reflected, returned to the retina, and, travelling again from the retina to the brain, renewed the sensation. I have ventured to call the phenomenon “Recurrent vision.”

It may be seen, with some difficulty, by the help of an induction-coil and Leyden jar; or even by simply charging a Leyden jar with an old-fashioned electrical machine and discharging it in a darkened room. The spark must be at least an inch in length; and, furthermore, the size and distance of the electrodes and the charge of the jar must be so adjusted that the discharge shall be *single*—a condition not always easy to fulfil with a short striking-distance.

Hanover, U. S., February 9, 1872.

-XLIV. *On the Origin of the Earth's Magnetism, and the Magnetic Relations of the Heavenly Bodies.* By F. ZÖLLNER\*.

1.

AT the last Meeting of the Royal Society, on the 25th of July, I had already the honour to communicate in broad outlines those views by means of which I derived from my researches "on the Law of Rotation of the Sun and the large Planets"†, an hypothesis on the physical cause of the earth's magnetism, and on its relation to the phenomena we observe on the sun's surface. It was then my intention not to publish my arguments before having developed them in such a manner, physically and mathematically, that I might deduce from them certain quantitative consequences which may be subjected to direct proof by observation. I was led by the theoretical deduction of the law of the sun's rotation to some formulæ which agree even better with the observed angle of rotation for different heliographic latitudes than the empirical formulæ which have been hitherto constructed, and I was therefore encouraged to subject my theory of the earth's magnetism to a similar examination.

I was, however, prevented till now by work of different kind from executing my plan; but as in the mean time some very interesting papers have appeared, the results of which are, I think, closely related to my theory, I have decided to publish its principles already in the following. I intend to examine it afterwards more closely in the manner mentioned above if this should not have been done in the mean time by others, which I can only desire in the interest of the subject and as a support of my theory.

2.

I have tried in the above-mentioned paper to show that, ge-

\* Translated by Arthur Schuster, from a separate impression, communicated by the Author, from the Proceedings of the Royal Saxon Society of Sciences, Oct. 20, 1871.

† Proceedings of the Royal Saxon Society of Sciences, Feb. 11, 1871.



nerally speaking, only four properties of the sun are necessary and sufficient to account for all essential phenomena hitherto observed on the sun. These properties are the following:—

1. Intense radiation of heat from the sun.
2. Its rotation.
3. The existence of an atmosphere.
4. The liquid condition of the solar surface.

The first three of these properties are to be regarded as facts proved by observation; and I deduced the fourth as a necessary consequence of the eruptive character of a great many protuberances\*.

Respighi, who has carefully observed numerous protuberances, has been led to consequences which agree perfectly well with the views which, as he expressly remarks, have been hitherto maintained by myself alone†, as regards the liquid condition of the sun's surface as well as the nature of the sun-spots‡.

\* See Proceedings of the Royal Saxon Society of Sciences, June 2nd, 1870. In my paper then communicated, "On the Temperature and Physical Condition of the Sun's Surface," I made the following remarks:—

"Without stepping beyond the range of known analogies, and therefore of conditions explanatory of cosmical phenomena, it is scarcely possible to find a cause for these eruptive protuberances other than that of a difference of pressure between the gases in the interior and those on the surface of the sun. The possibility of such a difference of pressure, however, necessitates the existence of a zone of separation between the interior and exterior masses of hydrogen, the latter of which have been shown to form an essential part of the solar atmosphere.

† M. Emile Gautier, also, up to the year 1869, considered the sun-spots to be solid slag-like masses. But in a paper published in the *Archives de Genève* (August 1869) he abandons this view in favour of a more nebulous nature of the sun-spots, influenced by the opinion of Secchi and Faye. A discussion of my law of the sun's rotation by E. Gautier, which I receive whilst this paper is being printed, closes, however, with the following words:—

"Notre observation ne porte du reste, que sur un détail peu essentiel à la théorie de M. Zöllner. Celle-ci n'en reste pas moins la seule jusqu'à ce jour, qui s'assimile d'une manière aussi complète aux circonstances connues de la physique solaire, sans être obligée de recourir à des suppositions tout à fait en dehors des notions générales admises dans la physique terrestre."

‡ Respighi, "Sulle Osservazioni spettroscopiche del bordo e delle protuberanze solari" etc., *Atti della Reale Accademia dei Lincei* nella sessione I., del 4 dicembre, 1870.

The following passages confirm what I said above:—

"La straordinaria violenza o velocità di tali eruzioni ci prova che questi gas debbono trovarsi ad uno stato di enorme tensione, e che perciò essi debbono trovarsi ivi imprigionati o compressi dalla resistenza, o dal peso di uno strato od involucro esteriore di conveniente spessezza e densità....

"Non resta quindi che la supposizione di uno strato o involucro liquido, col quale potrebbero conciliarsi tutte queste particolarità." (P. 41.)

"Quantunque io prevegga che queste conclusioni incontreranno forti ed autorevoli opposizioni, pure ho stimato opportuno di riferirle, perchè mi si

From the first two of the given properties of the sun follows, as I have shown elsewhere, the development of the general circulation of its atmosphere, in consequence of which the heated masses of gas rise at the equator and hence generate in the lower parts of the atmosphere polar currents, in the upper parts equatorial currents, which generally flow without disturbing each other\*. These currents exert a double reaction on the glowing liquid surface, viz., 1st, a thermal one, and, secondly, a mechanical one. In consequence of the first of these reactions, the poles are cooled down by the contact of the descending and relatively cooler equatorial currents; in consequence of the latter, currents are produced in the liquid surface by friction, which change the normal rotation of the globe into one which corresponds to the developed law of rotation. By a closer examination of the relative velocities of layers lying above each other, we come, as I have shown elsewhere (*l. c.* p. 76), to the remarkable result that within the liquid layer affected by the atmospheric currents *the velocity of rotation increases with increasing depth, so that the layers lying deeper are in advance of those lying above them.*

Hence the currents in the glowing liquid surface of the sun are directed from east to west relatively to the inner nucleus, which rotates like a solid globe; and the law of rotation is only a consequence of the retardation which the upper layers of the rotating globe suffer by friction against the polar currents.

It follows, therefore, that to the law according to which liquid layers of the sun's surface lying on the side of each other have greater rotation-angles near the equator than in higher latitudes, corresponds a similar law for layers lying above each other.

In fact the equatorial parts of the liquid solar surface are to the polar regions as the middle of a river is to the part which is near the borders. Also in the vertical direction a similar differ-

presentano come assai concordanti coi fenomeni delle protuberanze, e perciò sotto questo rispetto meritevoli di qualche considerazione: e perchè in parte almeno si accordano colle idee emesse da autorevoli scienziati, e principalmente dallo Zöllner." (P. 44.)

\* I am glad that the existence of these currents has now been proved by the observations of the position and inclination of protuberances. In their under part these eruptions are directed to lower latitudes, in the upper parts towards the poles. This is proved by numerous observations of Tacchini in Palermo. Professor Spörer, according to a private communication, has observed the same phenomenon; and he also considers it a proof of the currents which I had deduced theoretically. Dr. Vogel, of the observatory of Chamberlain von Bülow, and Secchi confirm the same thing.

ence in the velocity of the layers is observed, but in the inverse direction of that on the sun's surface.

## 3.

We have seen that the two causes, which alone are sufficient to produce the phenomena of motion observed on the sun's surface, are a continuous flow of heat into space (that is, a permanent loss of heat of the surface) and the rotation of the globe. The sun in its present condition loses its heat directly by radiation of the glowing liquid mass. In a later state, however, when the whole surface will be covered by a solid crust, the loss of heat of the liquid mass will take place by conduction. The inner parts will give up their heat to the cooler crust, which in its turn loses it again by radiation. Hence it is to be seen that immediately under the solid crust the essential conditions required for the circulation are preserved. The liquid masses cooled down by contact and conduction will sink down as before at the poles, giving rise to a polar undercurrent which is accompanied by an equatorial upper-current. This equatorial current will touch continually the solid crust, and suffer a westerly deflection analogous to that of the upper trade-winds. For the inhabitants of such a globe the direction of the currents in the liquid nucleus below their feet will result from an equatorial component and a component in the direction of rotation. If, for instance, our earth were a globe of such properties, these currents would have a south-westerly direction in the northern hemisphere, whilst they would have on the southern hemisphere a north-westerly direction.

## 4.

We must now inquire whether the motion in the liquid nucleus could make itself perceptible in any way on the surface. As long as the crust is sufficiently thin to be affected by the changes necessarily accompanying such a motion (as the ice covering a river is affected by the water flowing underneath), such mechanical influences could be observed and measured by instruments comparing the direction of gravity with that of a solid body rigidly connected with the crust.

But as the thickness, and hence also the relative rigidity of the crust, increases in such a manner that these mechanical influences could no longer be observed, except perhaps by very delicate instruments indeed, changes extending over a longer period and affecting the intensity of the whole streaming mass could still make themselves perceptible by thermic changes in the solid crust. For whatever may cause such an increase or decrease in the intensity of the whole streaming process in the



inner parts of a heavenly body, according to the principles of the conservation of energy, this change in the *vis viva* of the currents must be accompanied by an equivalent change in the mean temperature of the system. We can therefore generally say:—*Changes in the mean intensity of the streaming process are accompanied by changes in the mean temperature of the streaming masses, and the masses connected with them. An increase in the vis viva of the current implies a decrease in the temperature, and vice versa. These changes must necessarily take place simultaneously (at least only retarded by the conduction of the solid crust), as the first phenomenon is only another expression of the second one.*

There is still a third way by which we could show the existence of these currents—if they were to cause disturbances in the electrical equilibrium. If the direction of the electrical currents, which are perhaps produced, were to depend merely upon the direction of the streaming masses, magnetic phenomena would take place on the surface which would be connected by certain laws with the general character of the motion, and hence also with the circles and points whose situation depends on the rotation of the earth.

# 5.

If we ask whether by the known laws of electricity we have the right to assume that electrical currents are produced by currents of liquids, we must answer in the affirmative. We shall see that we may positively assert that *wherever a streaming motion of conducting and chemically decomposable liquids takes place, electrical currents are produced which are connected by certain laws with these streams.* The observed facts by which, I think, this assumption is not only justified, but, according to the laws of logical induction, necessary, are the electrical currents and their reciprocal phenomena discovered by Quincke in the year 1859, and called by him “diaphragmic currents”\*.

The fundamental fact which forms the starting-point for Quincke’s researches is expressed by him in the following words:—

“If pure water flows through a porous body, an electrical current is produced. I found and confirmed this fact by the following experiments.”

The direction of these electric currents depends, with all liquids which have been hitherto examined, only upon the direction of the streaming movement, while their intensity varies a great deal with qualitative changes of the liquid. The direction of the positive current is always the same as that of the

\* G. Quincke “On a New Class of Electric Currents,” Poggendorff’s *Annalen*, vol. cvii. pp. 1–47, and vol. cx. pp. 38–65.

flowing liquid, so that the platinum plates which are immersed in the liquid for the observation of the current act in such a manner that the plate first touched corresponds to the zinc end, the plate which is last touched to the platinum end of a Grove's battery. The electromotive force of these currents is very large. Using, for instance, a diaphragm of common quartz-sand, the electromotive force is 6.2 times as great as that of a Daniell's cell; so that Quincke, at the end of his paper, formed the idea of making such a source of electricity practically useful. He remarks as follows:—

“I have lastly tried to make use of the water-works of this town to produce an electric current, as the large electromotive force made me hope to obtain currents of practical application.”

The experiments made with an instrument constructed for this purpose under a pressure of about  $2\frac{1}{2}$  atmospheres and an hourly use of 5 cubic feet of water, showed only weak electric currents. Quincke explains this result partly by the salt contained in the water, and partly by oxide of iron present in the pipes.

The electromotive forces seem to be (according to the experiments hitherto made) independent of the thickness and surface of the porous walls, but proportional to the difference of pressure causing the flow of the liquid.

The origin of this source of electricity has, until now, not been reduced to the known disturbances of the electrical equilibrium.

Even not considering the possible connexions of this kind of current with the following phenomena, it is of the highest importance for the present considerations that in a common river these electric currents can be shown to exist if the two plates at the ends of galvanometer-wires are inserted one in the more quickly flowing middle of the river, the other near its borders\*.

It is, as already stated, quite immaterial what we assume to be the cause of these electric currents; it is sufficient for us that we can show their existence directly by observation. It is clear that these currents will still be generated if, instead of the metal plates of the above experiments, two rocks projecting into the current are in conducting communication by means of the earth.

\* Adie (Phil. Mag. vol. xxxi. p. 353, 1847). “Two slips of zinc cut side by side from the same sheet were placed in a running brook, the one opposed to a rapid part of the current, the other in a still place at the edge. Connecting these in usual manner with the galvanometer, there was a permanent deflection of  $25^\circ$ ; and on changing the respective places of the plates in the stream without disturbing their attachments to the galvanometer, the needles immediately passed to the opposite side of the card; in both cases the piece of zinc in the current acted as a negative or platinode plate.

6.

If by the above researches of Quincke the production of an electric current by the mechanical displacement of liquid particles has been proved, the reverse experiments, showing the generation of a streaming movement of a liquid by making an electrical current to pass through it, have long been known under the name of "electrical endosmose."

The experiments made for this purpose, however, only showed positive results if a porous wall retarded the movements of the liquids. Using tubes filled with liquid and passing an electrical current through them, no movement of the liquid could be shown, although Wiedemann in his researches on this subject only considered the porous wall to be a system of narrow tubes, and ascribed the negative results to the small quantity of electricity passing through the liquid.

Armstrong\*, the inventor of the steam electrical machine, was the first to observe the formation of a continuous current of water produced by an electrical current without a porous diaphragm. He joined two well-pointed glasses filled with water, and standing at a distance of 0.4 inch from each other, by a damp thread of silk: connecting one of the glasses with the boiler, which was negatively electrified, and the other with the earth, the water flowed in the form of a column in the direction of the positive current. Soon the silk thread was pulled into the glass which was joined to the earth, and hence moved in the opposite direction to the column of water. Then the water remained a few seconds, sometimes even a few minutes, in the form of an arc between the two glasses. During this time no change of volume of the liquids in either of the two glasses could be observed. When particles of dust were thrown over the surface of the water, they indicated a double current—an exterior current directed from the positive to the negative glass, and an interior current in the opposite direction.

A short time ago Quincke† not only confirmed this fact by careful experiments modified in many ways, but he also reproduced it by leading a strong current of voltaic electricity through capillary tubes. Quincke proved, by finely divided substances suspended in the moving liquids, quite generally the existence of a double current.

He introduced grains of starch into the liquid of a capillary tube of about 0.4 millim. diameter. When the liquid had entirely filled the tube, an electrical current was led through

\* *Phil. Mag.* vol. xxiii. p. 199 (1843). *Pogg. Ann.* vol. lx. p. 355.

† *Pogg. Ann.* vol. cxiii. p. 513 (1861).



the water by connecting the platinum wires melted into the tube with the conductor and cushions of the electrical machine.

In turning slowly the disk of the electrical machine, a movement of the particles of starch, in the direction of the positive current, was observed near the walls of the tube; in the middle of the liquid the movement was in the opposite direction. It was observed by a microscope magnifying thirty times. If the intensity of the current is increased, the particles in the middle of the tube move more quickly, while the larger particles near the walls change their direction and move now with the negative current. Increasing still more the intensity of the current, all the particles move in the direction of the negative current\*.

Constant galvanic currents and induction-currents which (by interrupting the communication by a column of air) are always directed in the same sense, act similarly; the particles of starch first proceed a little in the direction of the positive current, turn suddenly round and flow quickly in the opposite direction.

Using wider tubes, a stronger current is necessary to produce the motion of all the particles of starch in the same direction; with the narrow tubes very weak currents are sufficient.

These facts, which are described in detail in Wiedemann's 'Treatise of Galvanism and Electro-magnetism,' are sufficient to justify the assumption *that all streaming motion in liquids, especially if they are partly in contact with rigid bodies, is accompanied by electrical currents which are developed, as far as we can see by the facts known at present, chiefly in the direction of the flowing liquids.*

## 7.

Let us now apply these facts by analogy to the currents in the liquid nucleus of the earth which we considered above. The direction of the upper currents in the northern hemisphere is the same as that of the trade-winds, viz. south-westerly. Hence the direction of the electrical current produced in the earth's crust is a north-easterly. We must look on the inequalities in the inner crust of the earth, which are continuously touched by the glowing liquid, as on blocks of rock which on the bottom of the sea or of mighty rivers are always influenced in one direction by the flowing water. They act just in the same manner as the platinum plates do in the experiments of Quincke. The plate touched first by the current (the westerly projection) corresponds to the zinc, the plate touched last (the easterly projection) to the platinum of a Grove's battery. Hence the current in the wire (or in the earth's crust) has an opposite direction (*i. e.* from east to west).

\* Pogg. Ann. vol. cxiii. p. 569 (1861).

It is known that the assumption of electrical currents so directed accounts for the most general appearances presented by the earth's magnetism; at the same time its origin is assumed to be by this theory at a considerable depth below our feet—a circumstance very remarkable with reference to the theory of Gauss. Lamont, to whom we are so much indebted for the closer examination of the earth's magnetism, and who probably amongst living philosophers has the most competent judgment on this subject, makes the following remarks:—

“Gauss (and this is doubtless the most remarkable part of his theory) has directed our attention to a circumstance which must become of great importance for future researches; for under certain conditions we can draw from it conclusions which will allow us to say whether the seat of the magnetic force is above us in the atmosphere or below us within the earth, or on its surface. If we apply the rules flowing from this to the formulæ of Gauss, we shall find that the seat of the earth's magnetism, if not wholly, at any rate in its chief part is situated below the surface of the earth”\*.

It follows moreover, from the assumptions justified above for the physical cause of the earth's magnetism, that all causes and circumstances modifying the direction and intensity of the glowing liquid streams in the earth must necessarily also change the direction and intensity of the earth's magnetism.

Such causes are not only possible; they are necessary. In fact, if the streams alter the inner side of the earth's crust in a similar manner as the currents of water and air alter the side we inhabit, the form of the bed in which the glowing masses flow must change, and hence the intensity and direction of the currents must vary.

I believe we observe these continual changes in the so-called *secular variations of the earth's magnetism*.

About the attempts to explain these secular variations, Lamont expresses himself as follows:—

“If there are many difficulties in reconciling with each other the different peculiarities mentioned, so as to form a theory of the earth's magnetism, this becomes quite impossible as soon as we add the fact proved by observation, that the distribution of the magnetism within the earth changes sensibly from year to year, and not by starts, but by a continuous movement.”

“In order to explain these secular variations, some have said that some parts in the earth's liquid nucleus are solidifying and so modify the distribution of the magnetism; others have assumed the existence of an invisible magnetic planet revolving in

\* Lamont, *Astronomy and Magnetism of the Earth*. Stuttgart, 1851, p. 260.

seven hundred years round the earth. It is easily seen that nothing is gained by hypotheses which are neither supported by analogy nor facts, and at present the best thing is to confess that we cannot make any acceptable supposition about these mysterious phenomena."

We see from this that just that peculiarity which formed the most difficult problem for all explanations hitherto given is almost self evident, looking at it from the point of view of the above suppositions.

If the general direction of the glowing liquid streams agrees, as already said, with the conditions given by the position of the magnetic needle\*; certain physical consequences may be drawn from my theory for particular conditions by which the streams or their beds may be modified. We shall consider now more closely these consequences.

It is clear that the configuration and nature of the outer surface of the earth is in some connexion with the nature of its inner surface, although perhaps only within wide limits.

The ice which covers a lake or the sea while it is agitated by manifold movements and currents, is characterized on its surface by manifold inequalities, which in part correspond to similar inequalities on the inner side; and so may be the inner surface of the earth's crust. Where mighty mountains of heavy stones come forth on the upper surface, similar projections will probably appear on the inner surface; and these inequalities projecting deeply into the streams must have an influence on them, as the mountains at the bottom of the sea have an influence on the currents of the sea, or the mountains on the earth on the currents in the atmosphere.

## 8.

If these views are confirmed by nature, and if the solid crust covering our planet is not yet thick enough to cause these influences to disappear, we shall expect that the situation and configuration of large chains of mountains influence the direction in which the magnetic needle points, as they influence the direction of the pendulum.

Lamont, after having mentioned the subterranean experiments of Reich in Freiberg† and his own magnetic observations made on high mountains, says, with regard to this question:—

"On this occasion I may remark that I have confirmed by my

\* If we look at the position of the isoclinic lines, or those of equal horizontal intensity, or the lines of equal total magnetic intensity, all of them have, as regards direction, the character of strongly deviated equatorial currents in the sea or in the atmosphere.

† Reich "On Electrical Currents in Lodes," Pogg. *Ann.* xlviii. 1839.



own measurements the curious influence of the Tyrolese Alps on the direction of the magnetic needle, an influence which was first found by Kreil."

It may therefore be of interest to determine accurately the magnetic constants in places where the deviation of the pendulum is observed, in order to be able to find out a connexion between these phenomena. But such influences of mountain-chains may possibly be explained by the assumption of magnetic minerals contained in them. A treatise of Menzzer lately published, "On the Connexion of the Configuration of the Continents with the Situation of the Magnetic Poles of the Earth," is of great interest for this question. This paper will be found in Poggendorff's *Annalen*, supplementary volume v. p. 592, and is dated "Halberstadt, November 3, 1870." The number of the *Annalen* was published in the beginning of August 1871, and came into my hands on the 5th of August.

It is easily understood that some indications given therein, as well as the results arrived at, must be of the greatest interest for my theory, which I had already stated on July 25. In fact, we may even from some indefinite hints draw the conclusion that Dr. Menzzer was conducted to quite similar views as myself on the origin of the earth's magnetism.

So, for instance, at once in the beginning of the researches for the foundation of the mathematical formulæ we find the following passage:—

"We may deduce from the earth's rotation that the positive currents going round the earth from east to west are really the cause of its magnetic polarity; and we may even find the origin of these currents. But it is not at present my intention to give this deduction. What I want to show is merely how we can derive from these currents in the earth the situation of the magnetic poles.

"If the earth were covered throughout by a homogeneous solid crust, the currents connected with its rotation would go round the earth exactly from east to west\*; the result would be that the magnetic poles would coincide exactly with the geographical poles. This is approximately the case with that part of the earth's crust which reaches from below to about half the depth of the sea. But the part above consists of land and sea; and this circumstance modifies the electrical currents in such a manner that the same tendency which appears in the rigid part of the earth as the currents causes in the liquid part a real backward movement of the waters.

\* This fact would not be quite true according to the above cause of the currents; for their direction could only result from an easterly and polar component.

"The cause that the magnetic poles of the earth are shifted relatively to the geographical poles, lies in the different action of this backward tendency; and if the statement which has only been made as an assertion is true, we ought to be able to calculate the situation of the magnetic poles from the configuration of land and water."

It appears to me, from the indefinite expression "this backward tendency," that the author was not acquainted with the relations shown above to exist between electrical currents and currents of liquids. At any rate, the fact remains qualitatively correct; as to the sufficiency of the quantitative data for the explanation of the facts in question, I do not take upon myself to judge.

The result of the numerical calculation is certainly remarkable for the very exact coincidence with Hansteen's observations \*

At the end of his paper the author makes the supposition that, by a gradual rising or falling of the continents, a change in the position of the magnetic poles, and hence secular variations of the magnetic constants, may be effected. According to my theory, the distribution of heat and cold in the earth's crust, if with decided differences, ought to be of great influence. A great lowering of temperature on the surface will cause a more rapid cooling at the particular spot in the crust and thus cause in the liquid streams a tendency to descend. To such a tendency the poles of cold owed at an early phase their formation. I have shown in my paper on the Law of the Sun's Rotation that the distribution of heat on the sun's surface, as observed by Secchi, may be very simply deduced from the thermic reactions which the atmospheric currents exert on the glowing surface. As this distribution of temperature is always such that by itself it would produce the currents by which it is generated, any irregularities which at the earliest state of development had influenced these currents would increase with increasing lowering of temperature, and so fix permanently the position of the poles of cold. It is to be seen from this that generally these poles will not coincide with the poles of rotation, on the sun as well as on the earth. It follows that in these points the descending currents will predominate, and poles of currents will be formed which must coincide with the magnetic poles.

It is therefore evident that, also looked at from this point of view, permanent differences in the temperature of the earth's surface must, if great enough, have an influence on the inner currents, and hence also on the earth's magnetism.

\* Hansteen finds, from his observations, long. of the magnetic pole east of Ferro  $290^{\circ} 21'$ , lat.  $69^{\circ} 30'$ ; the calculations of Menzzer give long.  $289^{\circ} 37' 28''\cdot5$ , lat.  $69^{\circ} 11' 53''\cdot8$ .

Such differences may very well be caused by the earth's surface being covered with or bared of ice and water; and in this way the configuration of the continents may have a certain influence on the earth's magnetism\*.

9.

The facility with which the so-called magnetic disturbances and their connexion with volcanic processes are explained by the aid of my theory is of much greater importance than all the facts given above.

In fact, every sudden change in the velocity of the streaming masses must cause in them a wave-like disturbance and an analogous disturbance in the position of the magnetic needle; it does not matter whether this change is called forth by the breaking away and falling down of some parts of the earth's crust or by a volcanic eruption, or, finally, by earthquakes produced by one of these two causes. I cite again some passages from the above-mentioned work of Lamont which refer directly to the phenomena we are now discussing. He says, speaking of the character of the magnetic disturbances:—

“If a great disturbance takes place, there is produced, as regards declination, only an oscillation about the mean direction; the mean declination of the disturbed days does not differ from the usual means. It is otherwise with the remaining magnetic elements: every large disturbance causes a decrease in the horizontal intensity and an increase in dip; and usually several days pass before the mean position is again obtained. Herewith the often-noticed fact is connected that a disturbance of great amount repeats itself the following days, but coming always at an earlier hour than before and with decreasing strength.”

Imagine a large block of rock torn away at a certain hour by the glowing streams under our feet and disturbing the fluid mass by its sudden sinking and pendulum-like rising and falling. The propagation of such an undulatory movement and its periodical return with decreasing strength might perhaps be the cause of the above phenomenon.

The characteristic properties of all large disturbances mentioned above (viz. the decrease of horizontal intensity and increase of dip) are also, I believe, easily explained by my views.

Imagine the surface of a globe covered with insulated wires parallel to the circles of latitude. Let an electrical current be sent separately through every turn of the wire in the same direction. This globe will then represent the earth as it is surrounded by electrical currents; and a magnetic needle fixed on the surface

\* Lenz, “Researches on an irregular distribution of the Earth's Magnetism in the northern part of the Gulf of Finland,” *Mém. de St. Pétersbourg*, vol. iii. pp. 1–38.



of the globe will present the same general appearances as a magnet on the surface of the earth. Suppose, now, that in one of the circular wires surrounding the globe an alteration in the electric current takes place; the needle will then be influenced in the same way in which that particular turn of the wire would influence it if acting by itself alone. Suppose the turn of the wire in question is just below the freely suspended needle and the current is increased. We see at once that the horizontal intensity will be diminished and the dip increased. The direction of the disturbance will always be the same, if those currents are increased which are running through wires whose distance from the equator is smaller than that of the needle (*i. e.*, speaking of the earth, have a lower geographical latitude). A disturbance taking place in higher latitudes than that of the needle, must consist in a decrease of the current if the needle be deflected in the same direction as before.

If, therefore, the above rule given by Lamont, as a consequence of his observations in the temperate zone, is on the average to be considered correct, we may conclude from the developed theory that, if the cause of a magnetic disturbance is in higher latitudes, then the disturbance must consist in a decrease of the intensity of the subterranean currents; if the cause takes place in lower latitudes, it must consist in an acceleration of these currents.

## 10.

The probability of this assumption is considerably increased if we express mathematically the relative velocities of the glowing streams for different geographical latitudes.

As we have only to consider the equatorial upper current of the liquid mass, which by the rotation of the earth and friction against the inner surface of the crust is deflected analogous to the upper trade-winds, the same theoretical considerations may be used, *mutatis mutandis*, which I have employed for the deduction of the law of the sun's rotation\*. The conditions of the problem agree in this case even better with the reality, inasmuch as we have here to do with a solid and rigid surface, which influences by friction the current of the liquids.

Designating the loss of velocity which a liquid particle suffers moving from lat.  $\phi$  to lat.  $\phi + \delta\phi$  by  $dv$ , and putting this loss proportional to

1. The difference in velocity of two points in the concave surface of the sphere, of which the difference in latitude is  $d\phi$ ,
2. The area of the surface of friction for unity of mass,

\* Proceedings of the Royal Saxon Society of Sciences, Feb. 11, p. 54.

3. The coefficient of friction  $a$  between the liquid and the solid crust,

then we have, as in the above work, pp. 54 & 55, for the loss of velocity  $dv$  by the motion of a liquid particle into higher latitudes,

$$dv = Aav_1 \sin \phi \cos \phi d\phi; \quad . \quad . \quad . \quad (1)$$

where  $A$  signifies a constant determined by the nature of the body, and  $v_1$  the linear velocity of a point on the *solid crust* at the equator.

Hence the velocity which a particle loses going from the equator to lat.  $\phi$  is

$$Aav_1 \int_0^\phi \sin \phi \cos \phi d\phi.$$

The remaining component of velocity  $v_i$  will be

$$v_i = v_1 - \frac{Aav_1}{2} \sin^2 \phi, \quad . \quad . \quad . \quad (2)$$

if we assume the original velocity of the particle at the equator to have been equal to the velocity of the solid crust.

Now the velocity of a particle in the solid crust lat.  $\phi$  will be

$$v_e = v_1 \cos \phi. \quad . \quad . \quad . \quad (3)$$

Putting

$$\frac{Aa}{2} = p,$$

and subtracting (3) from (2), we have

$$v_i - v_e = v_1 [1 - (\cos \phi + p \sin^2 \phi)] \quad . \quad . \quad (4)$$

This expression represents the law according to which the difference in linear velocity of the inner glowing streams ( $v_i$ ) and the solid crust ( $v_e$ ) varies with the latitude. But this difference is evidently nothing else than the relative velocity of the glowing liquid with respect to the earth's crust.

The constant  $p$  will always have a mean value considerably less than (1); so that the above expression *will generally increase with increasing latitude and have a maximum value at the poles.*

As, according to the developed theory, the magnetic appearances on the surface of the earth are only effects of the streaming movement in the liquid nucleus, *the magnetic phenomena must increase in intensity with increasing latitude.* It follows from this that the peculiarities of magnetic disturbances characterized as above by Lamont must be much more intense in higher latitudes, and can generally only consist in a decrease of the intensity of the current, as this was supposed to be the case in the above

explanation of the observed decrease in horizontal intensity and increase in dip.

The increasing strength of magnetic disturbances with increasing latitude is mentioned by Lamont in the following words (*l. c.* p. 271):—"The magnitude of the movements increases gradually from the equator towards the north and south pole; in the equatorial zone only slight movements are observed."

Another remark about the magnitude of the movements of the magnetic needle in disturbances is to be found on the following page.

"I have seen myself changes in declination of 10 minutes of arc taking place in one minute's time. In our neighbourhood such rapid movements are extremely rare; in the polar regions, however, it is frequently the case that the movement of the instruments cannot any more be accurately measured. Bravais and his companions, in the North-pole Expedition, 1838-39, had several times opportunity to convince themselves of this fact. Already in Petersburg and Sitka very rapid movements are not rare."

## 11.

It is easily seen that the beginning of a disturbance must be observed simultaneously in all stations on the earth's surface; for the influence of the earth on the needle is the resultant of the whole magnetic action of the earth. Every change in any one of the components must make itself perceptible in our observations as a change in the resultant. It is otherwise with the strength and nature of a magnetic disturbance; for its intensity must evidently be greatest at a point which is nearest to the seat of the cause. According to our theory, such a cause would consist in the tearing away or settling down of a piece of rock, which modifies the velocity of the glowing streams at the particular spot, and hence also the magnetic action on the needle.

If we call to our minds the movements which, according to the laws of hydrodynamics and the observations taken under analogous conditions, can be noticed in liquids, it is easy to account, from our point of view, for the propagation of these motions.

Suppose a piece of rock breaks off from the solid crust of the earth just below our feet. It would, following the law of gravitation, sink into the liquid, and then, according to its specific gravity, either rise again or sink down into deeper parts of the liquid nucleus, where under the influence of the greater heat it would dissolve.

In both cases the hydrostatic equilibrium which has been suddenly disturbed will restore itself in the form of an undulatory motion, which, starting from the point where the disturbance has taken place, will propagate itself in all directions with a ve-



locity peculiar to the liquid—similar to the circular waves going out on a quiet surface of water from a point where a stone has fallen into the water.

It is evident that the velocity of propagation, as well as the form of the waves, will be essentially modified if the liquid is not quiet, but flowing with a uniform velocity.

In a direction perpendicular to the current the waves will propagate themselves with the velocity peculiar to the liquid; parallel to the current they will propagate themselves more quickly in the direction of the current than in the opposite direction. At the same time the form of the waves will be more extended and drawn out on the side of the centre of the waves situated in the direction of motion. The height of the waves will depend upon the borders of the liquid; it will increase where the liquid is hemmed in.

Let us now apply these simple laws of hydrodynamics, *mutatis mutandis*, to the glowing liquid in the nucleus of our earth. Other modifications of the wave, generated in the above manner, will appear in the direction of the circles of latitude than in the meridian. In the first direction the wave will proceed as in a circular channel on both sides of the disturbed spot, and cause at a point about 180 degrees distant from it the opposite phase of that by which the wave was originated. In the present case this would be a wave-mountain. But as a rising of the liquid necessarily increases the pressure against the solid crust, and must hence also increase the friction of the glowing mass flowing eastward, it is clear that an oscillation of the magnetic constants will take place parallel to the direction of propagation.

It is clear moreover that the direction of propagation of a liquid wave must be distinguished from the direction of the current as regards the electromotive action produced by it. The mechanical process on which, according to my views, the origin of the electrical current depends, consists in a relative displacement of parallel liquid threads taking place always in the same direction. By the researches of the brothers Weber it was directly proved that, in an undulatory motion of liquid bodies, the particles are moving in more or less closed paths of comparatively small radii of curvature. A wave-mountain of the glowing liquid, produced as described, cannot but cause an increase of the hydrostatic pressure in whatever direction it propagates itself. Hence all the actions caused by a change in the velocity of motion of the glowing liquids (as, for instance, the magnetic action) can only be increased by such a wave, as the hydrostatic pressure becomes greater, with which, according to the experiments of Quincke, cited above, the intensity of the diaphragmic currents varies directly.

Lamont, indeed, draws our attention to the fact that the causes which generate a daily period in the changes of the magnetic constants are as a rule only increased by the disturbances, so that the latter may be considered to be only reinforcements of the causes which generate the daily variations. The words of Lamont upon this subject are (in the work mentioned, p. 271) the following:—

“Every hour in the day has its own predominant disturbances; and in general they are only reinforcements of the daily variations. We find, for instance, that if the declination is in its westward motion, it sustains by the disturbing force an impulse towards the west, and if it is moving eastward it receives an impulse towards the east.”

The following remarks of Lamont seem to me to be equally correspondent to the above consequences of my theory, if we remember the influence of the time of the day:—

“If we compare the simultaneous observations made in Petersburg, Katherinenburg, Barnaul, Nertschinsk, Sitka, Makerstoun, which are all near the parallel of  $55^{\circ}$ , we find that whenever a large disturbance takes place in Petersburg, as often happens, the needle in Katherinenburg turns towards the same direction but describes a much smaller path; in Barnaul and Nertschinsk the movement is in the same sense but almost vanishes. Most likely further to the east it disappears entirely, and reappears in Sitka in the opposite sense. In Makerstoun the disturbance takes place in the original form, but less intense than in Petersburg.

“Almost all the more important disturbances manifest themselves in the manner described; and I think it highly probable that there do not exist more than one source, but that all the disturbances have the same origin and the same course; they are, however, as already stated, modified by the time of day.”

In a still more characteristic way the agreement of the nature of the disturbances, as deduced from my theory, for places in the same latitude, is shown by the following words of Müller\*:—

“For different places having nearly the same latitude but different longitudes, a connexion between the disturbances is found but in a different manner. If at any time a particularly strong disturbance take place at a certain spot, it will appear towards east and west in the same direction but with decreasing strength; at  $90^{\circ}$  to the east and  $90^{\circ}$  to the west from the place where the disturbance has its maximum at the same moment only very weak oscillations, if any, will be observed; on the other half of the parallel the simultaneous deflections have another direction,

\* *Kosmische Physik*, third edition, 1872, p. 761.

reaching an easterly maximum at  $180^{\circ}$  from the point where the westerly maximum appears."

12.

The modifications of a magnetic disturbance in the direction of the meridian are explained in the same satisfactory manner in their general features by the developed theory, as are the modifications for the same parallel.

If we remember that as the wave proceeds towards the poles it must be necessarily hemmed in between its borders, and that the velocity of the current becomes larger with increasing equatorial distance, it is clear that through the first circumstance the height of the wave proceeding towards the poles must increase, as the height of the tidal wave is increased by like causes on the surface of the earth. By the second circumstance the intensity of the disturbance is increased as well; so that a disturbance beginning in our latitudes will make itself more perceptible towards the north, and less perceptible towards the south, without changing its direction in the same hemisphere.

If a wave crosses the equator and propagates itself in the southern hemisphere, it is evident that it must here cause the opposite movement of the needle. For as such a wave can only increase friction, and by it the electrical current, the direction of which is determined by other circumstances, the same cause which induces the north end of the needle to sink in the northern hemisphere will in the southern hemisphere produce a sinking of the south end.

All these consequences are confirmed by observation. Lamont, comparing the graphical representations of observations made simultaneously in the same meridian, remarks, on a disturbance beginning on August 28, 1841, at 1 o'clock in the morning (p. 273):—

"If we go from the equator towards the north or south, the movements are continually increasing in size; the form remains in essentials the same.

"The southern and northern stations seem at first sight not to agree; but there is a perfect harmony if we observe that in the south the movements are in an opposite direction.

"It is to be noticed that the further we remove from the equator the more discrepancies in form appear. In this observation no stations situated far to the north or to the south have taken part, and we do not know what form the disturbance has taken towards the poles; however, it is known from other observations that in the polar regions the disturbances reach an extraordinary magnitude, and change entirely their form."

"As regards the magnitude of the disturbances, I have de-



duced a remarkable law from the observations. There is for Europe a certain scale of disturbances, according to which, if in Milan a movement of 10' takes place, the corresponding movement in Munich amounts to 11', in Krakau 12', in Breda 16', in Göttingen 18', in Copenhagen 22', &c. According to Bravais, the number of the scale for Bosskop would be 55'; but here the form is already changed so as almost to be no longer distinguishable."

The difference between the two hemispheres which we have deduced theoretically is, of course, independent of the cause which produces the increase or decrease in the magnetic action, whether this cause is an inner one as in the disturbances, or an outer one, as this is the case with the influences the sun exerts upon the earth. In accordance with this, Lamont says with reference to the daily periods:—

"In the southern hemisphere the succession of the magnetic changes during the twenty-four hours' period is exactly the same, but the movement is throughout in the opposite direction; where a westerly movement, or an increase, takes place in the north, an easterly movement, or a decrease, is found in the south. The movement is smallest to the south of the equator when it is greatest to the north, because the winter of the southern hemisphere corresponds to our summer." (*L. c.* p. 268.)

Finally, the following words of Lamont will support materially the views which I have explained regarding the connexion between the magnetic phenomena on the earth and the undulatory movement of a subterranean liquid mass:—

"However important the magnetic disturbances are for the theory, they would hardly have retained our attention so long, if they had not something peculiar, I might say something magical, inasmuch as they are called forth by invisible forces, not a trace of which is found to exist in the other phenomena presented by nature."

"It is even impossible to follow and take down by observation all the small deviations, especially the wave-like appearance which manifests itself in all magnetic changes."

"A uniform increase or decrease of the magnetic elements never appears; but the change takes place by starts, so that after every start a little retrograde movement is observed. We are at once reminded of the flux and reflux, where each subsequent wave goes a little further than the former did, and between the two waves a backward movement of the water takes place. The magnetic waves are besides as little like each other as the waves of the sea; probably they will also be different according to the geographical position. The passage of a magnetic wave in our country lasts about fifteen seconds. This remarkable peculiarity

of the magnetic force was noticed by me in the year 1841, when I had put up magnetical instruments of new construction and with very light needles which were enclosed air-tight; with the heavy bars formerly in use movements of such small durations could not be observed.

[To be continued.]

XLV. *On a Bicyclic Chuck.* By A. CAYLEY, F.R.S.\*

THE apparatus, although I have called it a chuck, is constructed, not for turning, but for drawing; viz. it rotates horizontally on a table (being moved, not from the inside by the axle of the lathe, but from the outside by a handle-frame), carrying a drawing-board which works under a fixed pencil supported by a bridge. Two points of the drawing-board describe circles; and the curve traced out on the drawing-board is consequently that described by a fixed point upon a moving plane two points of which describe circles; or, what is really the same thing, it is the curve described on a fixed plane by a point rigidly connected with two points each of which describes a circle. The apparatus is at once convertible into an oval chuck of nearly the ordinary construction; viz. it may be arranged so that the curve described on the drawing-board shall be an ellipse.

Bottom plane is a rectangular board (1) (see figure) about 30 inches by 24 inches, having in the middle a sliding-piece (2) carrying a block (3).

Second plane contains two circular segments (4) fixed to the bottom plane, serving as an axle for the moving piece (5) next referred to, and allowing the block (3) to move between them. And in the same plane we have a moving piece (5) in the form of a rectangle with a circle cut out thereof, rotating about the segments (4), and having upon it a groove in which works a sliding-piece (6) carrying a block (7); there is in this block a circular hole, D. The second plane includes also two sides (8) of a handle-frame, which two sides slide along two of the sides of the piece (5).

Third plane consists of a rectangular piece (9) rotating about an axle fixed to the block (3), and having a sliding-piece (10) in which is a circular hole, C. The third plane includes also the before-mentioned block (7), having upon it the hole D; and it includes also the remaining two sides (11) of the handle-frame, and, let into the same so as to be flush therewith on the upper surface, two slips (12) completing, in this plane, the handle-frame.

We have thus on a level the sides (11), (12) of the handle-frame and the holes C, D, where C rotates about the point B,

\* Communicated by the Author.

which is the centre of the block (3); and D rotates about the point A, which is the centre of the segments (4), each hole being capable of describing a complete circle; and the distances AB, BC, CD, and DA are (within limits) adjustable to any given values: the distance of the holes C, D is made equal to that of the two pegs next referred to.

Connected herewith by means of cylindrical pegs working in the holes C, D respectively, we have a carrying-frame; viz. the fourth plane contains two sides (13) of this carrying-frame, and two moveable bars (14), attached to the remaining two sides (15) of the carrying-frame, and having on their lower surfaces the pegs which work in the holes C and D respectively—each bar being free to rotate about one extremity, and being clamped at the other extremity so as to allow the two pegs to be adjusted at a given distance from each other. And then in the fifth plane we have the remaining two sides (15) of the carrying-frame.

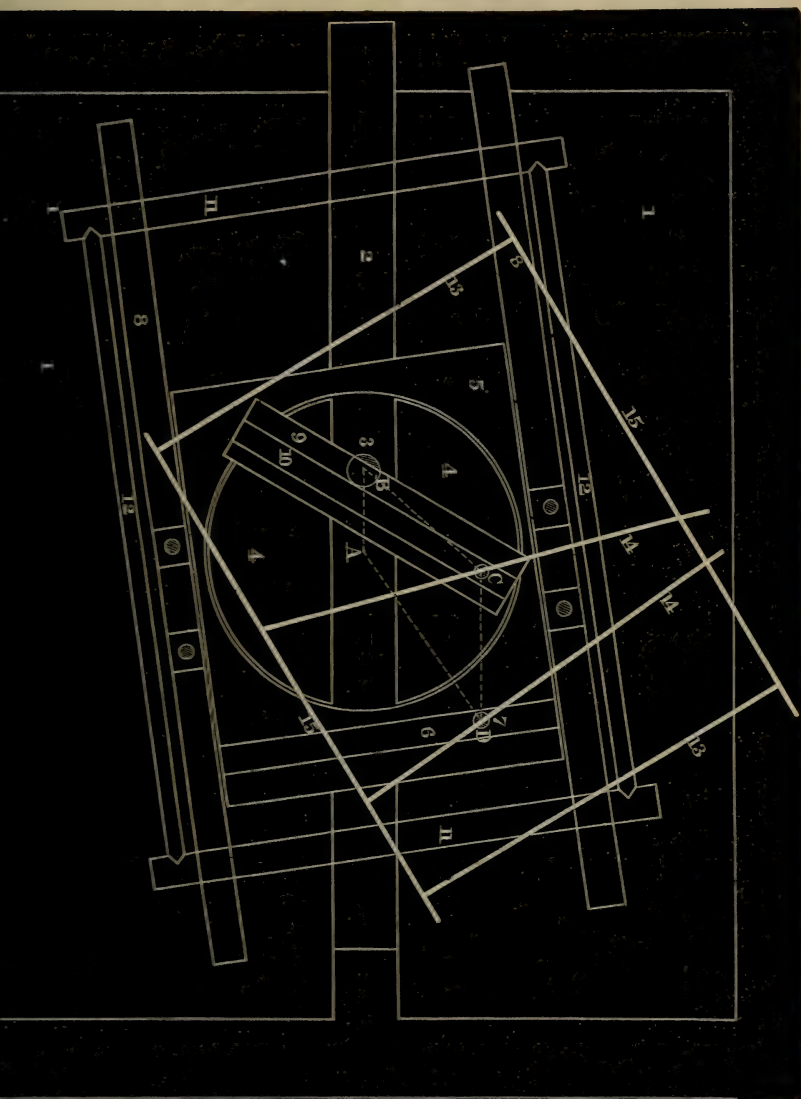
Rigidly connected with the carrying-frame we have the drawing-board; or, to make the whole more complete, this should be adjustable to any given position in regard to the carrying-frame by giving it two sliding motions crosswise, and a rotating motion, in the manner of an eccentric chuck.

To convert the apparatus into an oval chuck, we remove altogether the carrying-frame; and in the third plane we fix to the sides (8) of the handle-frame two bars at right angles to these sides, by means of pegs on the lower surfaces of these bars fitting tightly into holes on the sides (8) (which holes and the ends of the bars are shown in the figure), in such wise that these bars include between them the piece (9), which is thereby kept in a direction at right angles to the sides (8), and thus slides between the two bars. There are thus in the handle-frame two lines at right angles to each other, which pass through the fixed points A and B respectively; so that, now connecting the drawing-board directly with the handle-frame, the apparatus has become an oval chuck, viz. the curve traced out on the drawing-board will be an ellipse. The drawing-board should be adjustable to any given position in regard to the handle-frame, in like manner as it was to any given position in regard to the carrying-frame; it is easy to arrange as to this.

It is hardly necessary to remark that the pencil should have two sliding motions crosswise, so as to allow it to be adjusted to any given position; and a small up-and-down motion, so that it may be loaded to press with the proper force upon the drawing-board.

The variety of forms, even with a fixed adjustment of the chuck, only the position of the pencil being altered, is very considerable: among them we have bent ovals and pear-shapes, passing through cuspidal forms into bent figures-of-eight.





Third  
plane.  
Second  
plane.  
Bottom

XLVI. *On a Collector for Frictional Electrical Machines.*  
By Dr. H. EMSMANN\*.

FROM observing how effective the condenser is when applied to Ruhmkorff's coil, notwithstanding the small space that it occupies, I was led to imagine that a compendious *collector*, which would nevertheless increase the effect considerably, might, in like manner, be adapted to the ordinary frictional electrical machine.

In this I have been completely successful. At an inconsiderable outlay I have attained what the remarkable and comparatively expensive ring (now, however, disproportionately expensive) produces when adapted to Winter's electrical machine.

My original idea was to form the collector of a *single* long strip of tinfoil to be folded up between two overlapping strips of waxed paper, and which would be enclosed in a cover of similar material of about octavo size. I purposed placing this collector on the conductor itself of the machine (like Winter's ring), or on the metallic portion between the conductor and the points.

M. Kuhlo, the assiduous and skilful philosophical-instrument-maker of this place, to whom I addressed myself to carry out my idea, has now executed the *collector* in this way—namely, by inserting into each other several glass tubes hermetically sealed at one end, the diameter of the widest tube being about 2 inches.

With the exception of the widest, which serves simply as an insulating cover, all the tubes, varying in number from three to five, are coated externally with tinfoil applied with paste; the edges of their open ends lie in the same plane; and at those ends all these tinfoil coatings are united together and brought into metallic connexion with the conductor of the machine, the arrangement being such that the collector can be adapted thereto or removed at pleasure.

The effect produced is astonishing. The *collector* accomplishes even more than Winter's ring does when placed, instead thereof, upon a machine furnished with such a ring.

This simple and inexpensive *collector* can be easily adapted to any frictional electrical machine; so that with every such machine we may obtain in an easy and cheap manner similar effects to those for which Winter's ring was so remarkable.

Stettin, February 1872.

\* Communicated by W. G. Lettsom, Esq., from Poggendorff's *Annalen*, vol. cxlv. p. 332.

XLVII. *On Electrolysis, and the Passage of Electricity through Liquids.* By G. QUINCKE\*.

SOME time ago, in a paper "On the Transport of Material Particles by Current Electricity," I attempted to show how a series of phenomena of motion which current electricity brings into play may be simply explained if we assume that electricity is excited, not merely on the contact of two metals, but also when any two heterogeneous bodies are in contact. This assumption, which, I think, must necessarily be made so long as no definite limits can be drawn between various bodies as regards their electrical deportment, enables us to conceive the conduction of electricity in electrolytes in a manner similar to that in the paper I have mentioned. The following communication, which, with the exception of the experiments with Thomson's galvanometer, was written as long as six years ago, is a continuation of the former, and is intended to show how far concordance between theory and experiment can be demonstrated. Though, from our ignorance of the magnitude of the resistance of friction, and of the excitation of electricity on the contact of the molecules, we cannot predict all the phenomena of electrolysis, Faraday's law for instance (which objection, as far as I know, might be raised against all the theories hitherto proposed), yet we can do it for a great number of phenomena without having recourse to new hypotheses.

§ 52.

Let us first suppose an electrolyte of linear dimensions through which an electrical current is continually flowing in the direction of the positive,  $x$ . By electrolyte we are to understand a body which undergoes some change in its chemical condition in one or more parts when an electrical current traverses it. To fix our ideas, let us suppose that some salt (say, chloride of sodium in some solvent such as water) is the electrolyte.

No substance in nature is a perfect insulator. All substances without exception conduct electricity, like metals; only they oppose greater resistance to the motion of electricity than the metals. Whether a different kind of conduction (what is called the electrolytical) is possible, is a different question, which must be left to further investigation to solve.

It is assumed that each of the partial molecules of which the total molecule is made up has a definite quantity of free electricity; in this case it is each molecule of chlorine and each molecule of sodium: let each sodium molecule have the quantity

\* Translated from Poggendorff's *Annalen*, No. 9, 1871.



of free electricity  $\epsilon$ , and each molecule of chlorine the quantity of free electricity  $\epsilon'$ .

This free electricity arises from the different degrees of attraction which sodium, chlorine, hydrogen, and oxygen, being simultaneously in mutual proximity in space, exert upon the two electric fluids. As in the case of the excitation of free electricity by the contact of different metals, so in this case also the free electricity of each constituent may depend upon the temperature of the bodies in contact, and may alter with the condition of the solvent. The magnitude and signs of the masses of electricity  $\epsilon$  and  $\epsilon'$  remain to be determined, but may in general be either both equal or different.

Each of the two masses of electricity will be moved with a force

$$-\frac{dV}{dx}\epsilon \text{ and } -\frac{dV}{dx}\epsilon',$$

where the force is positive in the direction of the positive electricity, and  $V$  is the potential of free electricity for the corresponding section of the conductor. Within the same section of the linear conductor the potential  $V$  is constant, and, in electrolytes as well as in metals, depends only on the free electricity on the surface of the conductor (compare § 57 *et seqq.*). Within the conductor there is, indeed, free electricity on the various constituent molecules; but at the same place in space equal and opposite quantities of electricity lie so near each other that they exert equal and opposite actions, and thus their action on more distant particles of electricity is nullified. If a *constant* current of electricity traverses the conductor, it is quite immaterial for the present considerations whether the free electricity has collected on the external surface in consequence of a mechanical displacement of liquid particles, or from some other cause.

Since the quantities of electricity on the individual molecules can only move slowly or with difficulty from one molecule to another, the quantities of electricity  $\epsilon$  and  $\epsilon'$  will carry with them the constituent-molecules (ions) to which they adhere; and, owing to friction against the surrounding liquid, each will very soon acquire a constant mean velocity.

These constant velocities

$$v = -C \frac{dV}{dx} \epsilon \text{ and } v' = -C' \frac{dV}{dx} \epsilon' \quad . \quad . \quad . \quad (1)$$

will have the direction of the forces acting upon the electrical masses  $\epsilon$  and  $\epsilon'$ , and with these masses will change their sign—that is, their direction. The constant  $C$  which defines the velocity of such a material constituent-molecule and the adhering quantity of electricity  $\epsilon$ , besides depending on the magnitude of

the moving mass and on the friction against the surrounding liquid, also depends on the greater or less difficulty with which the electrical mass  $\epsilon$  is detached from the material constituent-molecule.

The force with which the constituent-molecules (in this case chlorine and sodium) are separated can be supposed proportional to the relative mean velocity of the two molecules, and for a given distance is

$$K = A(v - v') = -\frac{dV}{dx} (B\epsilon - B'\epsilon'), \dots \dots \dots (2)$$

where  $A$  and  $B$  are constants.

If  $q$  is the section,  $\lambda$  the conductivity of the entire liquid,  $i$  the intensity of the current, then\*

$$\frac{dV}{dx} = -\frac{i}{\lambda q} \dots \dots \dots (3)$$

and

$$K = \frac{i}{q} \cdot \frac{B\epsilon - B'\epsilon'}{\lambda}; \dots \dots \dots (4)$$

that is, the force which tends to separate the partial molecules or the so-called ions of the same electrolyte is greater the greater the density of the current, if by density we understand, as usual, the quotient of the current-intensity and the section.

Hence substances which for definite currents are insulators would become conductors if the density of the current  $\frac{i}{q}$  were suitably increased, and would undergo decomposition. The latter would occur provided the force  $K$  were greater than the attraction which the two partial molecules exert upon each other in consequence of chemical action.

### § 53.

Let us assume that the electrical current  $i$  in the linear conductor (the thread of liquid under electrolysis) arises from a Grove's battery of  $n$  cells. Let  $G$  be the electromotive force of one element,  $l$  the length of the thread of liquid traversed by the current,  $W$  the resistance of the metallic conduction and of the elements; then by Ohm's law,

$$i = \frac{nG}{\frac{l}{\lambda q} + W};$$

and this value inserted in equation (4) gives

$$K = \frac{nG}{l + W \cdot \lambda q} (B\epsilon - B'\epsilon'). \dots \dots \dots (5)$$

\* Pogg. *Ann.* vol. cxiii. p. 586, § 46; and Kirchhoff, Pogg. *Ann.* vol. lxxv. p. 191.

If the resistance  $W$ , as is frequently the case, is very small compared with the resistance of the electrolyzed column of liquid, the expression (5) passes into

$$K = \frac{nG}{l} (B\epsilon - B'\epsilon'). \quad . \quad . \quad . \quad . \quad . \quad (6)$$

*The force which tends to separate the partial molecules (the ions) increases with the electromotive force of the battery used, is inversely proportional to the length of the thread of liquid to be electrolyzed, but independent of its section and conductivity.*

If the resistance of the elements and the metallic conduction cannot be neglected in comparison with the resistance of the electrolyzed thread of liquid, the force  $K$  is still smaller than is given by expression (6).

The force depends here very much on  $\epsilon$  and  $\epsilon'$ . In the case of solid bodies, where the partial molecules are hardly or not at all mobile, and the constants  $B$   $B'$  have the value 0, we have also  $K=0$ , and there is no electrochemical decomposition of the total molecule.

#### § 54.

Since each chlorine- and each sodium-molecule moves with a certain velocity, the relative velocity of these partial molecules will be retarded by the attracting forces which chlorine and sodium exert upon each other. On the other hand, the attraction towards the adjacent sodium- or chlorine-particle of the next or preceding complete molecule will again increase this velocity to the same extent. It may hence be assumed that when the electrolyte is generally decomposed, and conducts electricity electrolytically, the particles will move past each other with a mean constant relative velocity.

If in the unit of volume of liquid there are  $a$  parts by weight of sodium and  $a'$  parts by weight of chlorine,  $a$  and  $a'$  are to each other in the ratio of their chemical equivalents. If  $M$  is the entire quantity of the one ion (sodium), and  $M'$  that of the other (chlorine), which are carried along in the direction of the positive  $x$  by the unit of length of the thread of liquid, we have

$$\left. \begin{aligned} M &= q\alpha v = -C\epsilon \cdot q\alpha \cdot \frac{dV}{dx}, \\ M' &= q\alpha'v' = -C'\epsilon' \cdot q\alpha' \cdot \frac{dV}{dx}; \end{aligned} \right\} . \quad . \quad . \quad . \quad . \quad (7)$$

or, placing for  $\frac{dV}{dx}$  its value from equation (3),

$$\left. \begin{aligned} M &= i\alpha \frac{C\epsilon}{\lambda}, \\ M' &= i\alpha' \frac{C'\epsilon'}{\lambda}, \end{aligned} \right\} . \quad . \quad . \quad . \quad . \quad . \quad (8)$$



where the values  $M$  and  $M'$ , just as the corresponding  $\epsilon$  and  $\epsilon'$ , may be positive or negative.

In the middle of the thread of liquid the composition and concentration are not changed: for each quantity  $M$  or  $M'$  which emerges from a section, the same quantity  $M$  or  $M'$  comes on the other side from the adjacent section. It is different at the electrodes, at the ends of the thread of liquid, where the electrical current enters or emerges. Here, according to the direction of the motion (or, what is the same thing, according to the sign of  $M$  or  $M'$ ), sodium- or chlorine-molecules may accumulate or be carried away.

For the moment we know nothing as to the sign and quantity of  $M$  and  $M'$ . Assuming now that both are positive, and the number of removed equivalents of sodium are greater than the number of removed equivalents of chlorine, or

$$M > \frac{\alpha}{\alpha'} M',$$

in the last section, in which the electrical current emerges from the thread of liquid,  $M'$  parts by weight of chlorine meet with  $\frac{\alpha}{\alpha'} M'$  parts by weight of sodium, and combine to form

$\left(1 + \frac{\alpha}{\alpha'}\right) M' = \mu$  parts by weight of neutral salt. There remain then in the last section,

$$m = M - \frac{\alpha}{\alpha'} M' \quad . \quad . \quad . \quad . \quad . \quad . \quad (9a)$$

parts by weight of sodium; that is, they are here separated on the positive side of the  $x$ , at the so-called negative electrode or cathode.

Simultaneously from the first section of the thread of liquid, in the unit of time,  $M$  parts by weight of sodium have come out, and, of the  $\frac{\alpha}{\alpha'} M$  parts by weight of chlorine which have been united with them, only  $M'$  parts by weight have been carried away. There thus remain  $\frac{\alpha}{\alpha'} M - M'$  parts by weight of chlorine uncombined at the positive electrode or anode; if this be designated by  $m'$ , we have

$$m' = -\frac{\alpha'}{\alpha} \left( M - \frac{\alpha}{\alpha'} M' \right), \quad . \quad . \quad . \quad . \quad (9b)$$

where the negative sign denotes that  $m'$  is separated at the anode on the negative side of the  $x$ .

Hence the first section of the thread of liquid has become

poorer by  $\left(1 + \frac{\alpha'}{\alpha}\right)M$  parts by weight of neutral salt. If, therefore,  $\mu$  or  $\mu'$  be the parts by which the liquid at the cathode or the anode has become richer in the unit of time, then

$$\left. \begin{aligned} u &= \left(1 + \frac{\alpha'}{\alpha}\right)M', \\ \mu' &= -\left(1 + \frac{\alpha'}{\alpha}\right)M. \end{aligned} \right\} \dots \dots \dots (10)$$

If  $M$  and  $M'$  are both negative, equations (9) and (10) still hold; only then  $m$  and  $\mu$  are negative,  $m'$  and  $\mu'$  positive; that is,  $m'$  is the quantity of partial molecules separated at the cathode,  $m$  that at the anode; at the cathode the concentration of the liquid has become smaller, but greater at the anode.

Equations (9) and (10) hold moreover for the case (and this, by the way, is the most frequent one) that the two partial molecules are moved to different sides by the electrical current, and that  $M$  and  $M'$  have thus opposite signs. If  $M'$  is negative,  $\mu$  is so also; that is, the liquid undergoes a smaller degree of concentration both at the positive and the negative electrode, apart from the quantity of liberated or decomposed partial molecules.

From equations (9) and (10) the following results ensue. If  $M$  and  $M'$  have opposite signs, then

$$M < m, \quad M' < m', \quad \dots \dots \dots (11a)$$

and the concentration of the liquid decreases. If  $M$  and  $M'$  have the same sign, then

$$m < M, \quad m' < M', \quad \dots \dots \dots (11b)$$

and the concentration of the liquid increases at one electrode and decreases at the other. The increase takes place at that electrode towards which the greater equivalent-number of partial molecules is driven by the current.

Both equations (9) may also be written thus,

$$\alpha'm = \alpha'M - \alpha M',$$

$$\alpha m' = -\alpha'M + \alpha M',$$

or, taking equation (7) into consideration,

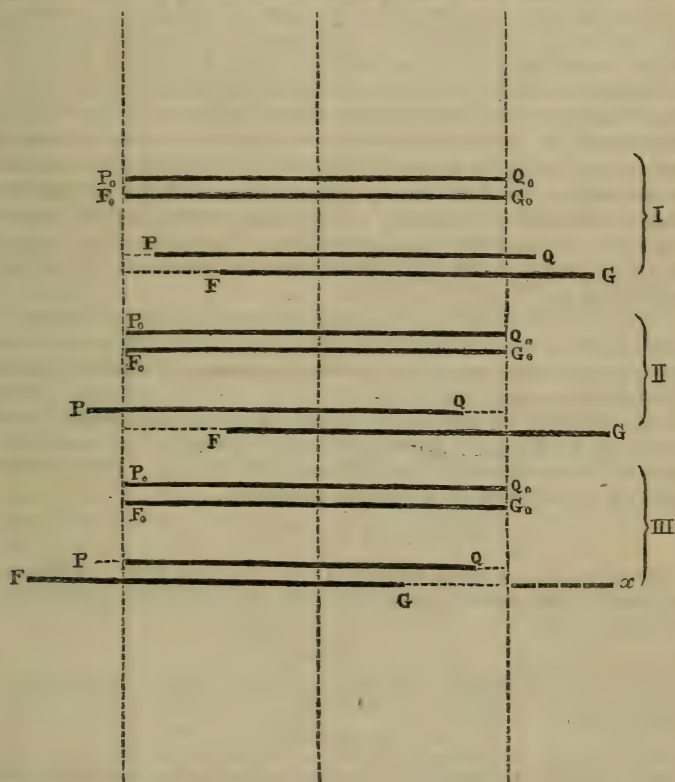
$$\left. \begin{aligned} m &= q\alpha(v-v'), \\ m' &= -q\alpha(v-v'); \end{aligned} \right\} \dots \dots \dots (12)$$

that is, *the quantities of the ions liberated are proportional to the relative mean velocity with which the partial molecules move past each other in the liquid.*

By the adjacent geometric construction an idea of the whole process may be obtained.

Let the line  $F_0 G_0$  represent the equivalent-number of sodium-molecules of the thread of liquid, and  $P_0 Q_0$  the equivalent-num-

ber of the corresponding chlorine-molecules before the electrical current is set up. After this has passed through the liquid for a second,  $F_0 G_0$  has come into the position  $FG$ ,  $P_0 Q_0$  into the position  $PQ$ . The middle dotted line at right angles to the  $x$  axis denotes any section, supposed to be fixed, towards which the displacement is observed. The distance of the points  $P_0 P$  or  $F_0 F$  measured upon the  $x$  axis represents the whole equivalent-number of partial molecules moved through the fixed section; the distance of the points  $PF$  or  $GQ$  the number of the equivalents separated at the anode or cathode; the distance of the points  $F_0 F$  and  $Q_0 Q$  the number of equivalents of salt by which the concentration at the electrodes has increased or decreased.



The three figures refer to:—

- I.  $M > 0$   $M' > 0$ ,
- II.  $M > 0$   $M' < 0$ ,
- III.  $M < 0$   $M' < 0$ .

[To be continued.]



## XLVIII. Notices respecting New Books.

*An Elementary Treatise on Curve-Tracing.* By PERCIVAL FROST, M.A., formerly Fellow of St. John's College, Cambridge, Mathematical Lecturer of King's College. London: Macmillan and Co. 1872 (8vo, pp. 208).

THIS work is almost exclusively devoted to the tracing of curves whose equations are of the form  $f(xy)=0$ , where  $f$  denotes a rational algebraical function. The author is perfectly aware of the limited aim of his book, and in fact says somewhat naïvely:—"The student might expect in a treatise upon this subject to find methods of drawing Polar Curves, Rolling Curves, Loci of Equations in Trilinear Coordinates, and Intrinsic Equations; he might also expect to find interesting Geometrical Loci discussed. These, and many other things immediately connected with the tracing of curves, have been deliberately omitted for reasons which I consider good" (p. v). These omissions are determined by the object for which the book is written, viz. to furnish preliminary exercises in elementary mathematics—especially in Algebra as far as the Binomial Theorem, the fundamental parts of the theory of Equations, and the general methods of Algebraical Geometry—with a view to assisting the student to acquire that mastery over these subjects which should be attained before he enters on the higher branches of mathematics, or on their application to Physical questions.

Of course the points in the theory of curves which are commonly given as applications of the Differential Calculus to Geometry occur here—such as drawing tangents and asymptotes, determining points of inflection, cusps, multiple points, radii of curvature, &c.; but as the equations of the curves are simple algebraical functions, these points can be discussed by successive approximations without reference to the methods of the Differential Calculus. Thus, let the equation to the curve be written in the form

$$u_1 + u_2 + u_3 + \dots = 0, \dots\dots\dots (1)$$

where  $u_1, u_2, u_3, \dots$  are functions of the 1st, 2nd, 3rd... order respectively; now, when  $x$  and  $y$  are both small, the first approximate value of (1) is

$$u_1 = 0,$$

which gives the tangent to the curve at the origin. The second approximation,

$$u_1 + u_2 = 0,$$

gives, generally, a curve of the second order approximating to the curve and having the same curvature at the origin as the given curve. It can further be shown that there are generally an infinite number of conics which have the property of coinciding with the curve up to the third order near the origin, and from amongst these the circle of curvature can be easily selected.

If we suppose the equation (1) to take particular forms, such as

$$u_1 w_1 + u_3 + \dots = 0,$$

$$v_1^2 + u_3 + \dots = 0,$$

$$v_1^2 + w_1^2 + u_3 + \dots = 0,$$

a similar process of reasoning shows that near the origin the curve in these cases has two branches with either distinct or coincident tangents, or has a conjugate point at the origin. It is scarcely necessary to observe that the student who has approached the subject in this manner will find that the corresponding parts of Treatises on the Differential Calculus offer no difficulty, and give in fact little more than rather obvious generalizations of points that have already engaged his attention.

The chief contents of the Treatise are these :—In addition to preliminary explanations, there is a full discussion of the forms of curves near the origin, and of the forms which they tend to take at an infinite distance, a subject which includes the discussion of asymptotes both rectilinear and curvilinear. There is also an account of various means (such as division into compartments) by which curves whose equations cannot be solved for one of the variables may be attempted “before they are given up in despair.” The properties of the “analytical triangle” are given at some length, as well as several illustrations of its ordinary use in ascertaining the form of curves whose equations are of a high degree. A novel use of the triangle is also suggested and illustrated by Mr. Frost, viz. as an aid in inferring the equation from the form of a traced curve. The work is illustrated by seventeen plates, which show the forms of a very large number (about two hundred) of curves whose equations are discussed in the text. Although the book is intended for those whose mathematical acquirements are limited, yet we presume it is addressed to students of more than average capacity. To such it will offer no serious difficulty; they will work through it in a short time, and in doing so will find much that is instructive, and many pleasant exercises of their ingenuity. To others, we fear, the study will prove a serious undertaking.

*Arithmetic in Theory and Practice.* By J. BROOK SMITH, M.A., LL.B. London: Macmillan and Co. (Crown 8vo, pp. 426.)

The aim of the author of the above work is to explain fully the principles on which the rules of Arithmetic are based, and to do this by numbers only without the aid of more general symbols. In carrying out this view, he states most of the principles of the science in the form of propositions, of which he gives formal proof. The aim and execution of the work are both excellent; but it may be noted that when a general statement has to be proved by reasoning on a particular case, the force of the proof is apt not to be felt by the student unless it is quite manifest that the reasoning which applies to one case will equally apply to all; and accordingly in several cases Mr. Brook Smith's proofs would have gained in clearness, if not in cogency, from the use of general symbols. Both as speci-

mens of the book and as instances of what we mean, we will cite two propositions.

*"If we interchange multiplicand and multiplier, the product remains the same.*

"Thus 7 multiplied by 5 is the same as 5 multiplied by 7. For write down 7 units in a horizontal line, and repeat this line 5 times; the number of units written down is 7 repeated 5 times or 7 multiplied 5 times. But in each vertical line there are 5 units, and there are 7 such lines; therefore the number of units written down is 5 repeated 7 times, or 5 multiplied by 7; that is, 7 multiplied by 5 is the same as 5 multiplied by 7." (P. 14.)

Here the reasoning is obviously conclusive; what is proved in the case of 7 and 5 is obviously true of any other two numbers. We will now take another proposition.

*"If the numerator and denominator of a fraction be prime to each other, the numerator and denominator of any fraction of equal value will be equimultiples of the numerator and denominator of the given fraction."*

"The numerator and denominator of  $\frac{4}{5}$  are prime to each other; and suppose  $\frac{8}{10}$  to be equal to  $\frac{4}{5}$ ; then 8 and 10 are equimultiples of 4 and 5.

"For, multiply numerator and denominator of  $\frac{4}{5}$  by 10, and of  $\frac{8}{10}$  by 5, these fractions will be unaltered in value; therefore

$$\frac{4 \times 10}{5 \times 10} = \frac{8 \times 5}{10 \times 5}.$$

And since the parts composing these fractions are all equal, the numbers taken in both cases must be equal, so that

$$4 \times 10 = 8 \times 5 \text{ or } = 5 \times 8.$$

And since 4 divides  $4 \times 10$ , it divides  $5 \times 8$ ; but 4 is prime to 5, therefore it divides 8; let the quotient be 2, so that

$$8 = 4 \times 2;$$

therefore

$$4 \times 10 = 5 \times 4 \times 2.$$

And dividing each of these quantities by 4, we have

$$10 = 5 \times 2;$$

that is, 10 is the same multiple of 5 that 8 is of 4; or the numerator and denominator of  $\frac{8}{10}$  are equimultiples of the numerator and denominator of  $\frac{4}{5}$ . (P. 63.)

In this case the reasoning is quite cogent, but is rendered obscure by the use of the second particular fraction  $\frac{8}{10}$ . We shrewdly suspect that a large number of students will remark when they come to the end of the proof, that it was plain to begin with that 8 and 10 are equimultiples of 4 and 5; *i. e.* they will have missed the point of the proof. And accordingly the proof would have been made



much plainer if the preliminary assumption had been

$$\frac{4}{5} = \frac{x}{y}.$$

The reasoning would then have run thus :

Therefore  $4 \times y = 5 \times x$ .

As 4 is prime to 5,  $x$  must be divisible by 4 ; let it equal  $4 \times m$ , then

$$y = 5 \times m.$$

Hence  $x$  and  $y$  are equimultiples of 4 and 5.

There is in reality no objection to the use of general symbols in a treatise on arithmetic. For it must be remembered that any student who takes in hand this or any other complete treatise will already be pretty familiar with the subject ; he will have begun learning it when a child, and have acquired his knowledge by a daily practice extending over some years. His object will be not to acquire knowledge of a new subject, but to extend, systematize, and render rational a knowledge which, as first acquired, was imperfect and empirical. For this purpose we know no better treatise than Mr. Brook Smith's. His explanations are clear and satisfactory ; he has gone with great thoroughness, though without prolixity, into the theory of the subject ; and he has illustrated all the parts of the book by an exceedingly large number of examples. A student who works straight through the book will go through an excellent preparation for passing any of the numerous examinations in which a thorough knowledge of arithmetic is required.

*Kuklos, an Experimental Investigation into the Relationship of certain Lines.* By JOHN HARRIS. Part First. Montreal : 1870. 4to, pp. 35. Ten Plates.

Though somewhat in doubt whether it were worth while to notice this book, we have on consideration determined to do so, for two reasons : in the first place, it is published in the dominion of Canada ; in the next, although it is undoubtedly an attempt to square the circle (and that is a very hopeless sort of undertaking), the author shows an acquaintance with the conditions of the question not to be generally found amongst his fellows ; for he allows that the ratio of the circumference to the diameter of a circle is correctly expressed by the number 3.14159 . . . (p. 12). Moreover the problem which he proposes to solve is not intrinsically absurd ; there must, of course, be some straight line intermediate in length to three and four times the diameter, which is exactly equal in length to the circumference ; and, further, the rectangle under half that line and the radius equals the area of the circle. The problem would therefore be solved if by a direct geometrical construction a straight line could be drawn equal in length to a given fraction of the circumference. Mr. Harris's notion of the solution seems to be this :—Let AB be an arc of a circle whose centre is O ; join OA ; draw AT, a tangent to the arc at A ; take AO<sub>1</sub> = twice AO ; with centre O<sub>1</sub>, draw an arc AB<sub>1</sub>, join O<sub>1</sub>B, and produce it to cut AB<sub>1</sub> in B<sub>1</sub> ; the length of the arc AB<sub>1</sub> is plainly the same as that of AB. If the construc-

tion be repeated, another arc  $A B_2$  will be found (whose radius is four times  $A O$ ) equal in length to  $A B$ . This construction can be repeated indefinitely, and thus a portion of  $A T$  is determined equal to  $A B$ ; this is what Mr. Harris calls the process of unbending an arc. Now the only remark we have to make on this is, that it is not a solution of the question,—it is not a direct geometrical construction; for it must be repeated an infinite number of times to yield the required result. As an approximation, when repeated a finite number of times, it is all very well, but of no particular interest.

This, as far as we can make out, is the one grain of sense in the book. For the rest, it is marked by the oddities which in one way or another the whole tribe of circle-squarers displays. Such is the picturesque description of the circle-squarer holding the unwilling mathematician “by his optical sensorium” (p. 7). The position of the introduction, which is printed by itself on the last page: and, in fact, the contents of the book generally, are odd; *e. g.* on p. 18 Mr. Harris gives what he calls “interrogative proposition X.,” which ends thus:—“Shall  $B H$ , the part so cut off from the straight line  $B D$ , be the required line equal in length to the arc  $B m$ ?” We are inclined to think he intends this solemn question to be answered in the affirmative; but whether this be so or not, there is no sort of doubt about the true answer:— $B m$ , an eighth part of the circumference, is 0.78540 times the radius, while an easy calculation shows that  $B H$ , determined by his construction, is 0.78361 times the radius.

On the whole, should the book fall into the hands of any of our readers, we can cordially advise them not to waste their time over it.

## XLIX. *Proceedings of Learned Societies.*

### ROYAL SOCIETY.

[Continued from p. 313.]

Nov. 23, 1871.—General Sir Edward Sabine, K.C.B., President, followed by Mr. Francis Galton, Vice-President, in the Chair.

THE following communication was read:—

“Note on the Spectrum of Encke’s Comet.” By William Huggins, D.C.L., LL.D., V.P.R.S.

I give the following observations of Encke’s comet, and of the spectrum of its light, in the order of the dates of the evenings on which they were made.

Oct 17. The comet presented the appearance of a nearly circular faint nebulosity, in which no condensation could be certainly distinguished.

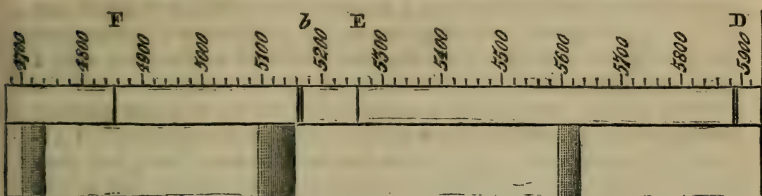
Nov. 7. By this time an important change had taken place in the appearance of the comet. There was now a strong condensation of light towards the east. The more condensed part of the comet, which was fan-shaped, was bounded on the eastern side by a tolerably defined contour, which approached in form to a parabolic curve. Surrounding this brighter portion of the comet was a much

fainter nebulosity, of which the boundary on the eastern side appeared to form a line at right angles to the axis of the comet.

I suspected a very minute stellar nucleus just within the eastern extremity of the brighter condensed part, and to a small extent north of the comet's axis.

Nov. 8. The description given yesterday is applicable to the comet tonight. The brighter part appears more defined and in stronger contrast to the fainter outlying nebulosity. The nucleus is now visible with certainty. On the western preceding side of the comet, the side towards the sun, the cometary light becomes gradually fainter and more extended until it is lost to view.

On this evening the light of the comet was examined by the spectroscope. The larger part of the light was resolved by the prism into a bright band in the green part of the spectrum. The band was defined at its less refrangible limit, and gradually faded towards the blue. The micrometer gave 5160 millionths of a millimetre as the wave-length of the less refrangible boundary of the band. Two other bright bands were occasionally suspected; one of them appeared to be about two thirds of the distance from the bright band towards D, the other a little distance beyond F. No continuous spectrum could be detected. The nucleus was probably



much too minute and faint to give a continuous spectrum that could be seen.

No difference in the spectrum was seen when the slit was moved over the comet in different directions, as far as its feeble light permitted.

The spectrum of a hydrocarbon, giving the bands which appear to be due to carbon, was then reflected into the instrument, and observed simultaneously with that of the comet. The band in the green was found to be identical in position with the brightest of the bands of carbon, and to be similar in gradation of brightness from its less refrangible limit.

Nov. 9. The observations of yesterday were confirmed. The second more refrangible band, which was then caught only by glimpses, was found to be coincident with the third band in the carbon spectrum. The wave-length of the less refrangible limit was about 4735<sup>mm</sup>. The least refrangible of the three cometary bands could be seen only occasionally.

Nov. 12. The observations on this evening contain no new facts.

Nov. 13. To-night the nucleus appears as a minute, well-defined stellar point.



In the spectroscope the three bands are distinctly seen. The position in the spectrum of the least refrangible band corresponds with the first band of the carbon spectrum; it commences from the red, with a wave-length of about  $5632^{\text{mm}}$ .

Attempts were made with a double-image prism, a Nicol's prism, and a Nicol's prism combined with a Savart's system of plates to detect polarized light in the comet, but without success.

Nov. 14. The form of the comet remains nearly the same. The outlying nebulosity is now chiefly on the south of the axis of the comet. The nucleus appears to be precisely at the extreme eastern limit of the brighter, more condensed part of the comet.

The same spectrum was seen, but fog coming on interrupted the observations.

On this evening an attempt was made again to detect polarized light. A double-image prism was placed between the eyepiece and the eye. The prism was brought into four different positions  $90^\circ$  apart. At each position of the prism an attempt was made to estimate the relative brightness of the two images. The power of the prism was just sufficient to give two images of the comet without their overlapping. The difference in brightness of the images was exceedingly small; I could not be certain that any appreciable difference really existed. However, I attempted in each case to select one of the two images as the brighter one. Afterwards I determined the position of the prism at the four different estimations, and I then found that three of the estimations were in accordance with a portion of the comet's light being polarized in a plane passing through the sun, and one in opposition to that supposition. I hesitate to attach any positive value to these observations; but they may perhaps be taken as showing that no considerable part of the comet's light is polarized.

The foregoing observations appear to show that the spectrum of this comet is identical with that of Comet II. 1868, a description of which I had the honour to present to the Royal Society\*.

It is worthy of notice that the cometary matter appears drawn out and diffused towards the sun, and that it has not yet come under the influence of the force, or been subjected to the conditions, whatever they may be, by which in most cases cometary matter appears to be powerfully repelled from the sun.

The observations were made with the telescope belonging to the Royal Society, of 15 inches aperture. The spectroscope contained one prism with a refracting angle of  $60^\circ$ , and the small observing telescope magnified six times.

Dec. 7.—George Biddell Airy, C.B., President, in the Chair.

The following communication was read :—

“On Fluoride of Silver.—Part III.” By G. Gore, F.R.S.

In this communication the author has finally shown that the action of iodine, under the influence of heat (including the process described by Kämmerer, *Phil. Mag.* 1863, vol. xxv. p. 213, for the isolation

\* *Phil. Trans.* 1868, p. 555 and plate xxxiii.

of fluorine), does not liberate uncombined fluorine, but produces fluoride of iodine and iodide of silver, a double salt, composed of iodide of silver and fluoride of platinum, being produced at the same time by corrosion of the platinum vessels, if the temperature approaches a red heat.

The fluoride of iodine produced is a highly volatile and colourless liquid, does not corrode mercury or red-hot platinum, corrodes glass at 60° Fahr., and crystals of silicon at a red heat, also platinum in contact with argentic fluoride in a state of fusion; it instantly turns a deal splint black, fumes powerfully in the air, and is decomposed with violence by water into hydrofluoric and iodic acids, in accordance with the following equation:— $\text{IF}_5 + 3\text{H}_2\text{O} = 5\text{HF} + \text{HIO}_3$ . It dissolves iodine, and is absorbed by that substance; it is also absorbed either by argentic fluoride or iodide when those substances are cooled in its vapour, and may be expelled from them at a red heat. Its vapour quickly darkens the colour of a deal splint, and very gradually turns paraffin brown.

The platinum vessels in which the reaction with iodine was effected were considerably corroded (but less so than when bromine or chlorine were employed); and many expensive vessels were rendered useless by this cause during the experiments.

No chemical change occurred on heating argentic fluoride to redness with pure carbon.

By heating this fluoride to redness in a current of dried coal-gas, it was wholly reduced to metallic silver, hydrofluoric acid and tetrafluoride of carbon being evolved.

In liquid cyanogen, argentic fluoride neither dissolved nor suffered chemical change; but at a low red heat, in a current of dry cyanogen gas, it was entirely reduced to metal, either nitrogen and tetrafluoride of carbon, or fluoride of cyanogen being liberated. An aqueous solution of silver fluoride was precipitated by passing a prolonged current of cyanogen gas through it. Fluoride of silver was also decomposed by fusion with paracyanogen.

Argentic fluoride was not dissolved or chemically changed by immersion in anhydrous liquified hydrocyanic acid; but by passing the dry acid in vapour over the red-hot salt, the latter was decomposed and metallic silver liberated. Aqueous hydrocyanic acid readily precipitated a solution of argentic fluoride.

Fluoride of silver was not decomposed by heating it to redness in an atmosphere of carbonic oxide or carbonic acid gases; but by prolonged passage of the mixed gases through an aqueous solution of the salt, a brown precipitate, soluble in aqueous hydrofluoric acid, was produced.

By fusing the fluoride in a current of vaporous terchloride of carbon, it was wholly converted into argentic chloride, the vessels being much corroded, and an insoluble double salt of platinum and silver formed. Similar results took place on using tetrachloride of carbon. Silver fluoride was insoluble, and remained unchanged in liquid tetrachloride of carbon at 60° Fahr.; and terchloride or tetrachloride of carbon had no chemical effect upon an aqueous solution

of the silver-salt. A solution of bromine or iodine in tetrachloride of carbon was quickly decolorized by agitation with small particles of argentic fluoride.

Crystals of boron did not decompose fluoride of silver at a low red heat, nor chemically change at 60° Fahr. an aqueous solution of the salt containing either free hydrofluoric or nitric acids.

Vitrified boracic acid violently decomposed fluoride of silver in a state of fusion, emitting copious white acid fumes; but it had no chemical effect upon an aqueous solution of the salt at 60° Fahr.

By placing crystals of silicon upon argentic fluoride in a state of fusion, they become at once red-hot, undergoing rapid combustion, and evolving fluoride of silicon. A lump of fused silicon slowly decomposed an aqueous solution of fluoride of silver, setting free metallic silver in crystals. Crystals of silver behaved similarly, but much more rapidly, and evolved abundance of gas if the solution contained free hydrofluoric acid; on adding nitric acid to this mixture, bubbles of spontaneously inflammable silicide of hydrogen gas were evolved and ignited.

Pure and dry precipitated silica added to fluoride of silver, at a temperature of low redness, evolved much heat, with violent action, and set free metallic silver.

No chemical change took place on passing fluoride of silicon over red-hot fluoride of silver.

Argentic fluoride in a state of fusion is rapidly decomposed by sulphur with evolution of heat; fluoride of sulphur is at the same time produced, and argentic sulphide formed. To ascertain whether fluoride of sulphur is a gas or a volatile liquid, an apparatus called a "gas-collector" was devised and employed, and a full description of its construction is given. By using this apparatus, substances may be heated without contact with the external air, and without subjecting the joints of the apparatus in which they are heated to leakage by expansion or contraction of the gaseous contents.

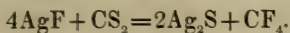
Fluoride of sulphur was found to be a heavy colourless vapour, uncondensable at the temperature of melting ice and at the ordinary atmospheric pressure. It corrodes glass, fumes strongly in the air, and has a characteristic and very powerful dusty odour, not very unlike that of a mixture of chloride of sulphur and sulphurous anhydride. Sulphur rapidly decomposed an aqueous solution of argentic fluoride.

Sulphurous anhydride passed over fluoride of silver at an incipient red heat, produced little or no decomposition of the silver-salt. Vaporous fluoride of sulphur also produced no visible effect.

By passing the vapour of liquid chloride of sulphur over the fluoride in a state of fusion, chemical action occurred, a vapour was evolved which corroded glass and possessed a dusty odour, but did not condense to a liquid; it was probably fluoride of sulphur. The saline residue consisted of argentic chloride and sulphide. A solution of argentic fluoride was decomposed by agitation with liquid chloride of sulphur, hydrofluoric acid being evolved, and argentic chloride and sulphide produced.



Argentio fluoride did not dissolve in bisulphide of carbon. By passing the vapour of the latter substance over the silver-salt at a red heat, a chemical change took place, and a fuming acid vapour was evolved, in accordance with the following equation :—



A solution of bromine or iodine in bisulphide of carbon was rapidly decolorized by agitation with particles of argentic fluoride, and the liquid acquired the odour of tetrafluoride of carbon.

December 21.—George Biddell Airy, C.B., President, in the Chair.

The following communications were read :—

“On some recent Researches in Solar Physics, and a law regulating the time of duration of the Sun-spot Period.” By Warren De La Rue, D.C.L., F.R.S., Balfour Stewart, F.R.S., and Benjamin Loewy, F.R.A.S.

1. In the short account of some recent investigations by Professor Wolf and M. Fritz on sun-spot phenomena, which has been published lately in the ‘Proceedings of the Royal Society’ (1871, vol. xix. p. 392), it was pointed out that some of Wolf’s conclusions were not quite borne out by the results which we have given in our last paper on Solar Physics in the Philosophical Transactions for 1870, pp. 389–496. A closer inquiry into the cause of this discrepancy has led us to what appears a definite law, connecting numerically the two branches of the periodic sun-spot curve, viz. the time during which there is a regular diminution of spot-production, and the time during which there is a constant increase.

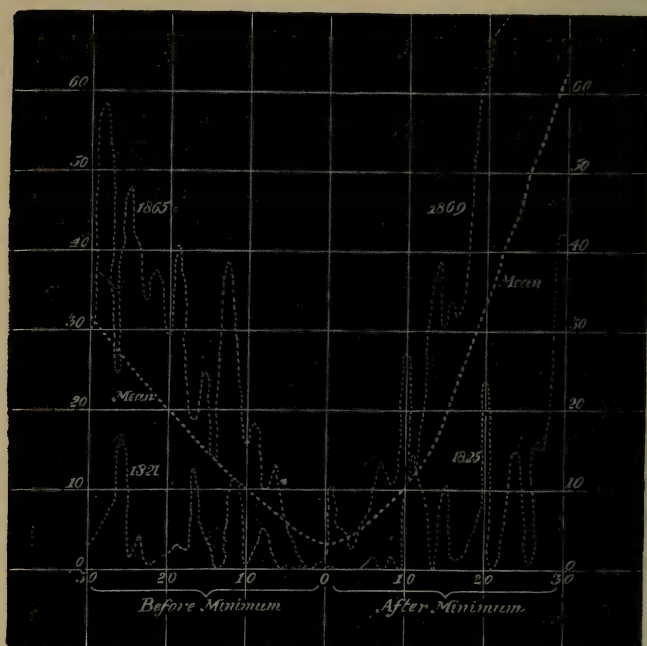
It will be well, for the sake of clearness, to allude here again, as briefly as possible, to Professor Wolf’s results before stating those at which we have arrived.

2. Professor Wolf had previously devoted the greater part of his laborious researches to a precise determination of the mean *length* of the whole sun-spot period, but latterly he has justly recognized the importance of obtaining some knowledge of the average character of the periodic increase and decrease. Hence he has, as far as he has been able to do so by existing series of observations, and his peculiar and ingenious method of rendering observations made at different times and by different observers comparable with each other, endeavoured to investigate more closely the nature of the periodic sun-spot curve by tabulating and graphically representing the monthly means taken during two and a half years before and after the minimum, and applying this method to five distinct minimum epochs, which he has fixed for the following years :—

1823·2  
1833·8  
1844·0  
1856·2  
1867·2

3. In a Table he gives the mean numbers expressing the solar activity, arranged in various columns, and arrives at the following results:—

(1) It is shown now, with greater precision than was previously possible, that the curve of sun-spots ascends with greater rapidity than it descends. This fact is shown in the subjoined diagram, which it may be of interest to compare with the curves given previously by ourselves in the above-mentioned place. The zero-point in this diagram corresponds to the minimum of each period; the abscissæ give the time before and after it, viz. two and a half years, or thirty months; the ordinates express the amount of spot-production in numbers of an arbitrary scale. The two finely dotted curves are intended to show the actual character of a portion of two periods only, viz. those which had their minima in 1823·2 and 1867·2; the strongly dotted curve, however, gives the mean of all periods (five) over which the investigation extends.



(2) Denoting by  $x$  the number of years during which the curve ascends and presuming that the behaviour is approximately the same throughout the whole period of 11·1 years as during the five years investigated, we have the proportion

$$x : 11\cdot1 - x :: 1 : 2,$$

whence

$$x = 3\cdot7,$$

or the average duration of an ascent is 3·7 years, that of a descent 7·4 years.

(3) The character of a single period may essentially differ from the mean; but on the whole it appears that a  $\left\{ \begin{array}{l} \text{retarded} \\ \text{accelerated} \end{array} \right\}$  descent corresponds to a  $\left\{ \begin{array}{l} \text{retarded} \\ \text{accelerated} \end{array} \right\}$  ascent. Thus the minimum of 1844·0 behaved very normally, but that of 1856·2, and still more that of 1823·2, shown in the above diagram, presents a retarded ascent and descent; on the other hand, in the minimum of 1833·8, and still more in that of 1867·2, also shown in the diagram, both ascent and descent are accelerated.

4. Finally, Professor Wolf arranged in the manner shown in the following Table the successive minima and maxima, in order to arrive at some generalization which might enable him to foretell the general character and length of a future period. Taking the absolute differences in time of every two successive maxima, and the mean differences of every two alternating minima, he shows that the greatest acceleration of both maximum and minimum happens together. This result strengthens our own conclusions, to be immediately stated, by new evidence, as it is derived from observations antecedent to the time over which our researches extend.

Minima.	Differences of alternating Minima.	Means.	Maxima.	Differences of successive Maxima.
1810·5			1816·8	
1823·2	23·3	11·65	1829·5	12·7
1833·8	20·8	10·4	1837·2	7·7
1844·0	22·4	11·2	1848·6	11·4
1856·2	23·2	11·6	1860·2	11·6
1867·2				

From this Professor Wolf predicts for the present period a very accelerated maximum—a prediction which seems likely to be fulfilled.

5. Comparing, now, M. Wolf's results with our own, it must not be overlooked, in judging of the agreement or discrepancy of these two independently obtained sets, that our facts have been derived from the actual measurement and subsequent calculation of the spotted area from day to day since 1833 recorded by Schwabe, Carrington, and the Kew solar photograms, which measurements are expressed as millionths of the sun's visible hemisphere, while the conclusions of M. Wolf are founded on certain "relative numbers," which give the amount of observed spots on an arbitrary scale, chiefly designed to make observations made at different times and by various



observers comparable with each other. This will obviously, in addition to the sources of error to which our own method is liable, introduce an amount of uncertainty arising from errors of estimation and the possibility of using for a whole series an erroneous factor of reduction. Nevertheless we shall find a very close agreement in various important results; and this seems a sufficient proof of the great value and reliability of M. Wolf's "relative numbers," especially for times previous to the commencement of regular sun observations.

6. The following is a comparison of the data of periodic epochs, as fixed by ourselves and M. Wolf :—

		I.	II.	III.	IV.
Minima epochs.	{ De La Rue, Stewart, and Loewy . . }	1833·92	1843·75	1856·31	1867·12
	{ Rudolf Wolf . . }	1833·8	1844·0	1856·2	1867·2

		I.	II.	III.
Maxima epochs.	{ De La Rue, Stewart, and Loewy . . . . }	1836·98	1847·87	1859·69
	{ Rudolf Wolf . . . . }	1837·2	1848·6	1860·2

It will be seen from this comparison that only one appreciable difference occurs, viz. in the maximum of 1847, which M. Wolf fixes nearly one and a quarter years before our date.

The mean length of a period is found by us to be 11·07 years, which agrees very well with M. Wolf's value, viz. 11·1 years.

7. We found the following times for the duration of increase of spots during the three periods, and for the corresponding decrease, or for ascent and descent of the graphic curve, beginning with the minimum of 1833 :—

	Time of ascent.	Time of descent.
I.....	3·06 years.	6·77 years.
II.....	4·12 „	8·44 „
III.....	3·37 „	7·43 „
Mean....	3·52 „	7·55 „

Professor Wolf gives 3·7 years and 7·4 years for the ascent and descent respectively; and considering that he derived these numbers only from an investigation of a portion of each period, the agreement is indeed surprising, and would by itself suggest that the times of ascent and descent are connected by a definite law.

8. M. Wolf has expressed in general terms the following law with reference to this relation of increase and decrease of spots :—

“The character of a single period may essentially differ from the mean behaviour; but on the whole it appears that a { retarded } descent corresponds to a { accelerated } ascent.”

We, on the other hand, have, by an inspection of our curves (*vide*

Phil. Trans. 1870, p. 393), been induced to make the following remark on the same question:—

“We see that the second curve, which was longer in period as a whole than either of the other two, manifests this excess in each of its branches, that is to say, its left or ascending branch is larger as a whole than the same branch of the other two curves, and the same takes place for the second or descending branch. On the other hand, the maximum of this curve is not so high as that of either of the other two; in fact the curve has the appearance as if it were pressed down from above, and pressed out laterally so as to lose in elevation what it gains in time.”

Although both statements appear to lead up to the same conclusion, viz. that ascent and descent are connected by a law, still they differ essentially in this respect, that if A, B, C represent the three following consecutive events, descent, ascent, descent, Professor Wolf's law refers to the connexion between A and B, while our remark refers to B and C. We consider two successive minima as the beginning and end of a single period, while M. Wolf, at least in this particular research, places the minimum within the period, and compares the descent from the preceding maximum with the ascent to the next one.

9. We have considered the connexion thus indicated of sufficient importance to apply to it the following test. If, using the previous notation, a definite relation exists between A and B, the *ratio* of the times which the events occupy in every epoch ought to be approximately constant; similarly with respect to B and C; and this ratio should not be influenced by the *absolute* duration of the two successive events. It is clear that the greater uniformity of these ratios will be a test for their interdependence. The following is the result of the comparison:—

a. Professor Wolf's law : comparison of A and B.

	Periods.	Duration of descent (A).	Periods.	Duration of ascent (B).
I.	1829·5 to 1833·8	4·3 years	1833·8 to 1837·2	3·4 years.
II.	1837·2 to 1844·0	6·8 „	1844·0 to 1846·6	2·6 „
III.	1846·6 to 1856·2	9·6 „	1856·2 to 1860·2	4·0 „

	Ratio $\frac{A}{B}$ .	Difference from mean.
I. ....	1·265	—0·728.
II. ....	2·615	+0·522.
III. ....	2·400	+0·307.
	Mean 2·093	

These differences from the mean are so considerable, that in the present state of the inquiry a connexion between any descent and the immediately *succeeding* ascent appears highly improbable. A very new and apparently important relation seems, however, to result from a similar comparison of any ascent and the immediately succeeding descent, or between B and C.

## b. Comparison of B and C.

Periods.	Duration of ascent (B).	Periods.	Duration of descent (C).
I. 1833·92 to 1836·98	3·06 years	1836·98 to 1843·75	6·77 years.
II. 1843·75 to 1847·87	4·12 „	1847·87 to 1856·31	8·44 „
III. 1856·31 to 1859·69	3·38 „	1859·69 to 1867·12	7·43 „

	Ratio $\frac{C}{B}$	Difference from mean.
I.....	2·212	+ 0·061.
II.....	2·044	— 0·107.
III.....	2·198	+ 0·047.
	Mean 2·151	

The agreement of these ratios with each other, and the small differences from the mean of the single ratios, justify us in the mean time, until a greater number of periods are before us, to state the connexion between the two branches of the periodic curve from one minimum to another in the following more precise terms:—

*If T be the time of duration of sun-spot increase from a minimum to a maximum, then  $2·15 \times T$  (with a probable error of less than  $\pm 0·05$ ) will be the duration of the sun-spot decrease until the next minimum.*

This law, together with the fact which we have previously established, that a longer period shows generally a depressed curve, while a shorter is characterized by great peaks, points strongly to the conclusion that *the energy of the ultimate causes of sun-spot production, whether these causes be intrasolar or extrasolar, is for every period constant.*

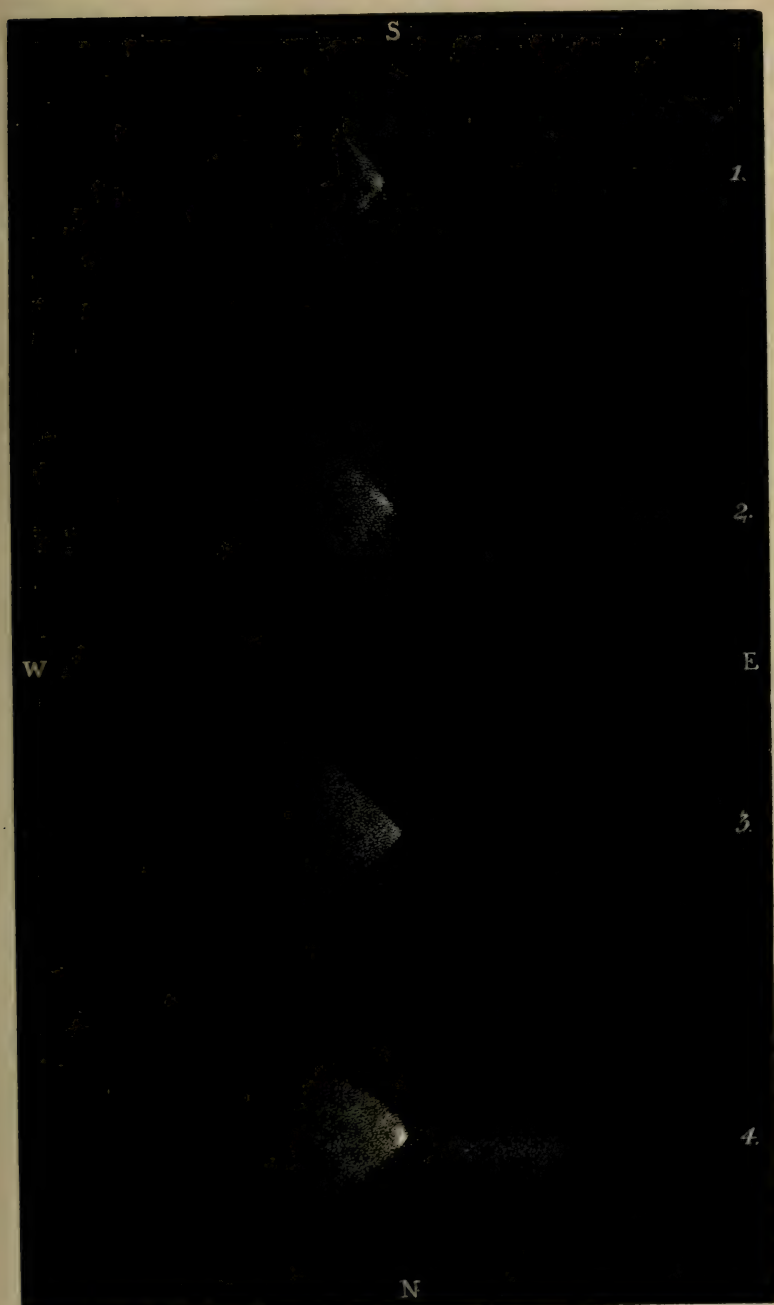
“Note on the Telescopic Appearance of Encke’s Comet.” By William Huggins, D.C.L., LL.D., F.R.S.

The first three figures which accompany this note represent the comet on evenings on which its appearance was described in a note on the spectrum of the comet which I had the honour to present to the Royal Society\*. A continuance of bad weather prevented me from making later observations of the comet, with the exception of one evening, December 5, when figure 4 was obtained under unfavourable circumstances.

Fig. 1. November 7, 7.30 p.m.—From Oct. 17, when the comet consisted of a nearly round nebulosity without condensation in any part, to Nov. 7 no observations could be obtained. At the latter date, the remarkable fan-form which distinguishes this appearance of the comet was already distinctly presented. The faint light by which the comet was surrounded terminated on the side from the sun, that from which the tail is usually projected, in a straight boundary at right angles to the longer axis of the comet. At the opposite side, that towards the sun, the faint nebulosity expanded and became fainter until it could be no longer traced. The minute stellar nucleus

\* Proc. Roy. Soc. vol. xx. p. 45.





which was suspected at the eastern extremity of the fan is not marked in the figure.

Fig. 2. November 8, 7 P.M.—The fan was now brighter and more defined in form. The nucleus, as a minute bright point, appeared to be situated not at the extreme western point, but a little within it, towards the north.

The sides of the fan were slightly curved, suggesting an approach to a parabolic form.

The fan was brighter on the southern side. The eastern edge of the faint light by which the comet was surrounded still preserved a right line from north to south.

Fig. 3. November 14, 6.40 P.M.—The appearance of the comet was essentially the same as on Nov. 8.

The bounding lines of the fan were perhaps less curved; they enclosed an angle of from  $85^{\circ}$  to  $90^{\circ}$ .

The nucleus had become brighter, and now appeared to form the extreme eastern point of the fan.

No prolongation of the eastern boundary, where the tail is usually formed, was seen.

Fig. 4. December 5, 5.30 P.M.—Thin mist in the atmosphere allowed the brighter parts only of the comet to be satisfactorily observed.

The condensation of light was now much stronger at the eastern end, but a defined nucleus was not detected.

The fan form was less marked; the brighter part of the comet more resembled a brush-like flame.

The atmospheric haze nearly concealed the faint light surrounding the comet, but, by glimpses, a tail was now seen to project towards the east; it was traced to a distance of about twice the length of the bright brush.

The tail appeared to come from the northern side of the longer axis of the comet, and to consist of a faint ray with sides nearly parallel.

As I am at present without a suitable micrometer, I was not able to take measures of the comet.

Jan. 11, 1872.—George Biddell Airy, C.B., President, in the Chair.

The following communication was read:—

“Experiments made to determine Surface-conductivity for Heat in Absolute Measure.” By Donald M'Farlane.

The experiments described in this paper were made in the Physical Laboratory of the University of Glasgow, under the direction of Sir William Thomson, during the summer of 1871. A set of similar experiments were made in 1865; but being merely preliminary, carried on by different individuals, and embracing only a limited range of temperatures, it is thought unnecessary to allude further to them here\*.

\* These experiments consisted of two series, one with the air moist by a little water placed in the interior of the vessel, the other having the air dried by

A copper ball, 2 centimetres radius, having a thermo-electric junction at its centre, was suspended in the interior of a double-walled tin-plate vessel which had the space between the double sides filled with water at the atmospheric temperature, and the interior coated with lamp-black. The other junction was in metallic contact with the outside of the vessel, and the circuit was completed through the coil of a mirror galvanometer. One junction was thus kept at a nearly constant temperature of about  $14^{\circ}$  Cent., while the other had the gradually diminishing temperature of the ball.

Having adjusted the galvanometer to the degree of sensitiveness desired, the copper ball was heated in the flame of a spirit-lamp till its temperature was considerably above that required to throw the spot of light off the scale; it was then put into position in the interior of the tin-plate vessel, and as soon as the spot of light came within range, the deflections from the zero position were noted at intervals of one minute exactly till the change of deflection was reduced to about two scale-divisions per minute.

Two series of experiments were made in this way, each consisting of several sets of readings. In the first the ball had a bright surface, and in the second it was coated with a thin covering of soot from the flame of a lamp, and in both the air was kept moist by a saucer containing a quantity of water placed in the interior of the tin-plate vessel.

As the range of differences of temperatures of the junctions extended over  $50^{\circ}$  Cent., the change in the difference of thermo-electric qualities of the copper and iron wires forming the junctions was very considerable, and it was necessary to make a careful thermometric comparison of the temperatures of the junctions and galvanometer deflections. For this purpose the junctions were tied to the bulbs of two previously compared thermometers, having their stems divided to tenths of a degree Cent.; these were then placed in two vessels of water, one at the temperature of the air, and the other heated by small additions of hot water, and kept well stirred; simultaneous readings of the thermometers and galvanometer deflections were then taken at various points of the scale\*, from which the formula

$$y = 0\cdot0924 + 0\cdot000227x$$

---

substituting sulphuric acid for the water in the first; and the results in the two cases were so nearly alike, that any effect due to the moisture or dryness of the air could not be distinguished from errors of observation. From this circumstance, as well as the limited range of temperatures, these results are not given here.

\* These readings were plotted, and the curve drawn through the points agreed very closely with a portion of a parabolic curve whose equation is

$$y = 2\cdot4 + 10\cdot6x - \cdot019x^2,$$

$y$  denoting the deflections of the galvanometer, and  $x$  the difference of temperature;  $y$  is a maximum when  $x = \frac{10\cdot6}{\cdot038} = 279^{\circ}$ , and, the colder junction having

been at  $16^{\circ}$  Cent., we get  $295^{\circ}$  as the neutral point of the specimens of copper and iron wires used—a very close agreement with former observations, considering the great distance of the neutral point from the temperature of the observations.



was obtained, where  $y$  is the value of a scale-division in terms of a degree Centigrade, and  $x$  the galvanometer deflection; and the difference of temperature of the junctions is therefore

$$xy = 0^{\circ} \cdot 0924x + 0^{\circ} \cdot 0000227x^2,$$

from which the numbers in col. II. of the following Tables were calculated.

The method adopted in reducing the observations was this:—Each single set of readings was arranged in a vertical column, and the whole series placed side by side with corresponding numbers in the same horizontal line; the means of the horizontal lines were formed into a similar column, and divided into groups, each consisting of four consecutive numbers, and the means of these groups form the numbers in col. I. of the Tables.

Col. II. contains the differences of the temperatures of the junctions at intervals of four minutes, corresponding to the mean deflections in col. I.

Col. III. contains the common logarithms of the numbers in col. II.

Col. IV. contains the differences of the successive numbers in col. III. divided by 4.

Col. V. is formed from col. IV., by multiplying by the Napierian logarithm of 10, and is the rate at which the difference of temperature varies per minute.

Col. VI. shows the quantity of heat emitted from the ball in gramme-water units per square centimetre per second per degree of difference of temperatures, and is formed by multiplying the numbers in col. V. by 009385\*, a constant depending on the surface of the ball and its capacity for heat.

The numbers found in cols. VI. and VII. were plotted on squared paper, and a mean curve drawn through the points; and, assuming the quantity of heat emitted to be represented by the formula  $x = a + bt + ct^2$ , where  $t$  is the difference of temperature, the coordinates of the curve were employed to determine  $a$ ,  $b$ , and  $c$ ; and col. VIII., calculated by the formula, is added to show the degree of approximation to which the results of the experiment are represented by it.

\* The surface of the ball was 50·26 sq. centimetres, and its capacity for heat 28·31 gramme-water units. Let  $x$  denote the heat emitted per second, per sq. centimetre per degree of difference of temperature, and  $C$  the rate at which the difference of temperature varies per minute; then

$$\frac{x \times 60 \times 50 \cdot 26}{28 \cdot 31} = C,$$

and therefore

$$x = \cdot 009385 C.$$

First Series.

Atmosphere moist. Copper Ball polished bright.  
Means of nine sets of Observations.

I. Mean of sets of observa- tions.	II. Difference of tempera- ture, D.	III. $\log_{10} D$ .	IV. $\log_{10} D' - \log_{10} D''$ 4	V. $\log_e D' - \log_e D''$ 4 C.	VI. Heat emitted per second by observa- tion.	VII. Mean difference of tempera- ture, $\frac{1}{2}[D' - D'']$ , (t).	VIII. Heat emitted per second, calculated by formula, (x).
19	66.88	.82527	.01037	.02387	.000223	63.83	.000230
28	60.79	.78380	.01045	.02406	.000226	58.00	.000227
82	55.21	.74200	.01055	.02429	.000227	52.65	.000226
50	50.10	.69981	.01041	.02396	.000225	47.81	.000224
14	45.51	.65815	.01033	.02378	.000223	43.45	.000222
15	41.38	.61681	.01028	.02366	.000222	39.51	.000220
37	37.64	.57570	.01015	.02337	.000219	35.96	.000217
26	34.28	.53509	.01020	.02348	.000220	32.75	.000215
64	31.21	.49429	.00992	.02284	.000214	29.85	.000213
89	28.48	.45459	.00982	.02261	.000212	27.25	.000210
42	26.02	.41529	.00943	.02171	.000204	24.94	.000208
58	23.85	.37756	.00962	.02215	.000208	22.84	.000205
96	21.83	.33907	.00945	.02175	.000204	20.92	.000202
15	20.01	.30127	.00915	.02106	.000198	19.20	.000200
20	18.39	.26467	.00905	.02083	.000195	17.62	.000198
57	16.92	.22845	.00909	.02093	.000196	16.24	.000196
99	15.56	.19207	.00901	.02074	.000195	14.94	.000194
51	14.32	.15603					

Formula for calculating column VIII. :—

$$x = .000168 + .00000198t - .000000017t^2.$$

Second Series.

Atmosphere moist. Copper Ball blackened.  
Ten sets of Observations.

I. Mean of sets of observa- tions.	II. Difference of tempera- ture, D.	III. $\log_{10} D$ .	IV. $\log_{10} D' - \log_{10} D''$ 4	V. $\log_e D' - \log_e D''$ 4 C.	VI. Heat emitted per second by observa- tion.	VII. Mean difference of tempera- ture, $\frac{1}{2}[D' - D'']$ , (t).	VIII. Heat emitted per second, calculated by formula, (x).
85	67.44	.82893	.01511	.03478	.000326	63.06	.000328
46	58.68	.76849	.01510	.03477	.000326	54.87	.000328
22	51.06	.70808	.01488	.03426	.000322	47.79	.000325
27	44.52	.64856	.01476	.03399	.000319	41.69	.000321
42	38.86	.58950	.01460	.03362	.000315	36.42	.000315
94	33.98	.53122	.01433	.03300	.000310	31.88	.000311
20	29.78	.47392	.01391	.03202	.000301	27.99	.000303
11	26.20	.41830	.01377	.03170	.000297	24.64	.000298
12	23.08	.36324	.01358	.03126	.000293	21.72	.000292
65	20.37	.30899	.01325	.03050	.000286	19.20	.000287
59	18.03	.25600	.01311	.03018	.000283	17.00	.000283
14	15.98	.20358	.01290	.02970	.000279	15.09	.000279
16	14.19	.15198	.01258	.02896	.000272	13.41	.000274
46	12.64	.10165	.01265	.02902	.000272	11.94	.000271
29	11.25	.05104	.01230	.02832	.000266	10.70	.000268
97	10.04	.00186	.01236	.02846	.000267	9.50	.000265
47	8.96	.95240	.01261	.02903	.000272	8.47	.000262
59	7.98	.90195					

Formula for calculating column VIII. :—

$$x = \cdot 000238 + \cdot 00000306t - \cdot 000000026t^2.$$

The following Table gives the results calculated by the formula for every fifth degree within the limits of the experiments :—

Difference of temperature.	Heat emitted.		Ratio of emissive power of polished to that of blackened surface.
	Polished surface.	Blackened surface.	
5	·000178	·000252	·707
10	·000186	·000266	·699
15	·000193	·000279	·692
20	·000201	·000289	·695
25	·000207	·000298	·694
30	·000212	·000306	·693
35	·000217	·000313	·693
40	·000220	·000319	·693
45	·000223	·000323	·690
50	·000225	·000326	·690
55	·000226	·000328	·690
60	·000226	·000328	·690

### *L. Intelligence and Miscellaneous Articles.*

ON CALCULATING-MACHINES. BY THOMAS T. P. BRUCE WARREN.

IF we review the history and fate of the numerous contrivances which have from time to time been proposed for economizing mental labour in calculating, we cannot be surprised on finding that such contrivances, however perfect, should be suspiciously regarded, and that their adoption should be somewhat slow.

The abacus, which is supposed to be the invention of Pythagoras, is probably the earliest recorded contrivance for calculating ; and, with a trifling modification, the Romans are found to have adopted it. The schwan-pan of the Chinese may probably claim a greater antiquity than either the Grecian or Roman abacus.

The invention of logarithms by Napier proved an inestimable boon to computers. Analogous to Napier's rods are the different forms of sliding-rules now in use.

Pascal's machine, although deficient in speed and accuracy, is said to contain the germ which has characterized all the later machines of the same class, namely those which give their results by the purely mechanical action of geared wheels, levers, or friction-rollers.

During the seventeenth and eighteenth centuries we find the names of Leibnitz, Grillet, and Sir Samuel Moreland amongst others who directed their attention to the production of calculating machines, which either obviated some of the disadvantages in the machine of Pascal, or possessed new features in their application to special operations.



The inventions of Mr. Babbage may be regarded rather as stupendous exercises of ingenuity than of ordinary practical utility.

The Swiss machine of Schultze, which is regarded as a completed idea of Mr. Babbage's original plan, has the disadvantages of great costliness and requiring skilled mechanical manipulation.

The arithmometer of M. Thomas de Colmar, with the exception of being extensively used by actuaries, is comparatively unknown, although its first appearance dates nearly forty years back.

The machine of M. Maurel, another French invention, appears to have existed only to share the fate of these machines generally. The invention of Staffel was tolerably near perfection in the rapidity with which it was capable of performing arithmetical operations.

Roth's automaton calculator was introduced about the year 1845; and no doubt, being a slight modification of the machines already noticed, it was capable of performing the same operations. M. Slovinski in 1849 introduced to the notice of the British Association a calculating-machine which was said to be perfect and reliable in its action.

Sang's platometer was, I believe, devised entirely for actuarial purposes, but has fallen into disuse from the uncertainty of its rolling and sliding motion.

With the exception of the arithmometer of Thomas de Colmar, I am not aware of any important application being made of these machines. It combines all that can be desired by the general computer; and the object of the author is to point out the fact that very material assistance may be expected from it in almost any calculation in applied mathematics. In a paper which was read before the Society of Telegraph Engineers, it was shown by the author that electrical Tables involving complicated calculations were capable of being worked out by it with astonishing rapidity.

As a matter of course, calculating-machines designed to combine the desiderata of portability and cheapness must be contracted in their range of performance; and consequently it devolves upon the ingenuity of the computer to bring his formula within the range of the machine.

A machine which can be depended upon for performing accurately the addition or subtraction of constant or variable quantities can be made to include the solution of problems where several intermediate results are required. Where only one result is required, machine assistance cannot be thought of; but when it is desired to tabulate a series of results, the time spent on arriving at a general solution so as to be presentable to the machine is compensated for in the relief of mental strain and the certainty of its accuracy.

The machine appears to be capable of giving assistance in the calculations which are involved in almost any branch of physical or applied science, such as the compilation of Tables of the expansion of liquids and gases by heat, the rarefaction and condensation of gases, specific heat and latent heat of bodies referred to volume under variable pressures, diffusion-tables, volumetric calculations, meteorological reductions, and so on.

General Hannyngton has shown that the machine of De Colmar can be employed in the construction of astronomical and nautical Tables.

Looking at the fact that calculating machines have been long felt to be a desideratum, and the barren results which have unfortunately followed the labours of most of the ingenious pioneers in the achievement of their perfection, I cannot help thinking either that sufficient confidence cannot be obtained in favour of machine-calculation, or that they are not placed before us with the *récommandations* they deserve.

Tamworth House,  
Mitcham Common.

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#### ON A NEW METHOD OF MEASURING THE VELOCITY OF ROTATION.

BY PROF. A. E. DOLBEAR.

While experimenting with the gyroscope, I have often wished to know its velocity, but knew of no way to determine it when it was set in motion in the usual way with a string. I have lately found a simple and exact way of doing this; and a description of the plan may be of interest to others, as it can be used to measure the velocity of wheels of every size and every possible speed without inconvenience and without expense.

If a short piece of wire be soldered to the end of one branch of a common tuning-fork, one end of the wire projecting a little on one side, and the fork made to vibrate at the same time that the point of the wire is drawn over a piece of smoked glass, an undulating line is made; and if the rate of vibration of the fork is known, the velocity of the moving hand can be found by counting the number of undulations in an inch on the glass. This has been used to determine linear velocity; but it can also be applied to rotary with great precision. I have a large fork with branches 8 inches in length that vibrates 171 times in a second as determined by a resonant tube; it has copper wire fastened to one branch and projecting about one-fourth of an inch,—made for showing to a class the waving line upon smoked glass. A brass disk, 4 inches in diameter, mounted gyroscopically, was smoked upon one side and set revolving by a string. The vibrating fork was then brought to it in such a manner that its vibrations were along the radius of the disk, but as soon as it touched it bounded away, and nothing could be determined with it. A *cone of india-rubber*, about a quarter of an inch in height, was then fastened with sealing-wax near one end of the branch, and this tried as before, with entirely satisfactory results, the markings being well defined and unmistakable.

When the number of vibrations the fork makes a second is known and the number of undulations made once round the disk, the former number divided by the latter will give the rate per second of velocity. Thus, if the fork made 100 vibrations in a second and the disk turned round but once in the same time, it is evident that there would be a hundred undulations on it; and if the disk turned fifty times a second, there would be but two undulations for each revolution.

In any case, if  $a$  equals the number of vibrations per second of the fork, and  $b$  equals the observed number of undulations in a single turn of the disk, then  $V = \frac{a}{b}$ .

A single wave, or even half of one, is sufficient for determination if the length be measured in degrees, in which case if  $d$  = the length of one wave, in degrees, the formula will stand  $V = \frac{ad}{360}$ . If the rotation be very rapid, the quickest possible touch is needed, or the undulations will return and confuse each other; but this is not troublesome at any speed I have been able to obtain, which is in the neighbourhood of 90 per second.

I have found that a common pocket tuning-fork, either an A or a C, answers very nicely with one of india-rubber fastened as before. So far as I now know, the swiftest motion that has been given to a disk was that in Foucault's apparatus for showing the motion of the earth, which he estimated at from 150 to 200 revolutions per second. Such a velocity would be recorded by from two to three undulations of a C fork, making 512 vibrations per second. If it would not be best to smoke the disk, a piece of white paper can be pasted upon it and smoked without burning; and it answers every purpose. To the certainty and ease of this method may be added another advantage, that the slightest touch needed for this cannot sensibly retard the motion of the disk, as any mechanical fixture attached for such a purpose must do.—Silliman's *American Journal* for April 1872.

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PRELIMINARY NOTE ON A REMARKABLE FACT OBSERVED ON THE CONTACT OF CERTAIN LIQUIDS OF VERY DIFFERENT SUPERFICIAL TENSIONS. BY G. VAN DER MENSBRUGGHE.

*Whenever a liquid of strong superficial tension, containing gases in solution, is brought into contact with a liquid of feeble tension, there is a more or less pronounced disengagement of the gases dissolved in the former liquid.*

This principle, which I publish now to secure date, intending, however, to verify it in detail in a special memoir, can be demonstrated by a great number of experiments. Provisionally I will only mention a few.

I. When a drop of alcohol or ether is introduced into distilled water half filling a phial of 3 or 4 centims. diameter and the liquid agitated, a brisk effervescence ensues. This experiment was long since described by M. Duprez\*, but without explanation. It is impossible to attribute the effervescence to air introduced by the agitation, since neither alcohol or ether alone nor water alone gives in this respect any marked result.

The experiment succeeds equally well with benzole, sulphide of carbon, creosote, oil of turpentine, olive, lavender, linseed, or colza oil, petroleum, oil of sweet almonds, &c. One has even merely to agitate distilled water, after having immersed in it a glass rod bear-

\* *Bull. de l'Acad. Royale de Belgique*, 1838, ser. 1. vol. v. p. 402.



ing traces of any fatty substance whatever, to see a distinct production of minute gas-bubbles.

If the phial containing the distilled water is not perfectly cleared of every fatty or etherial matter, numerous gas-bubbles soon form at the parts of the inner surface to which that matter is attached.

II. A drop of oil spreading on the surface of distilled water produces a disengagement of little gas-bubbles readily observed with a microscope; this disengagement I believe to be the true cause of the formation of what Mr. Tomlinson terms *cohesion-figures*—that is to say, of the division of the spread film into an infinity of parts constituting at first a sort of network, and decomposing, little by little, into lenticles of less and less dimensions, until, the gaseous disengagement ceasing, the minute lenticles remain indefinitely. With the microscope I was able to follow all the phases of the phenomenon, evidently due to innumerable minute gas-bubbles disengaged beneath the films.

The experiment can be made with any of the oils, fixed or volatile, with sulphide of carbon, wood-spirit, &c.

When any oil whatever is kept in prolonged contact with water, the surface of separation of the two liquids soon loses its transparency. This fact, so well known, is explained by the disengagement of minute gas-bubbles, which more or less resinify the oil and unfit it for transmitting light.

III. It was long since observed that water commences ebullition with more difficulty the better it is freed from the gases it holds in solution. What precedes enables us to foresee that by mixing distilled water with alcohol a great quantity of the dissolved gases can be expelled. This, in fact, is confirmed by an experiment recently made by M. Kremers: having added one part of spirit of wine to three parts of water and strongly heated, he saw the boiling-point readily rise to  $109^{\circ}$  C., and even far beyond, according to the proportion of the volatile liquid evaporated. I regard this experiment as a very curious verification of my principle.

Liquids of feeble tension favour the disengagement of bubbles of vapour as well as of gas; this is demonstrated by the striking experiments of Mr. Tomlinson. He has observed that greasy bodies prevent "*soubresauts*," while solid bodies perfectly free from grease do not at all produce the same effect.

IV. It is known that the Brownian or molecular movements are produced most energetically in a mixture of distilled water and any volatile liquid: in this case the movements appear to me to be a very plain consequence of my general proposition. As to their existence in a homogeneous liquid, we require to know whether the microscopic particles whose feeble trepidations have been seen were not more or less greasy; in that case they would necessarily give rise to a gaseous discharge, and consequently change their position from time to time. If the corpuscles are absolutely pure, they cannot manifest the little movements in question; moreover several observers have never been able to verify the Brownian displacements in a homogeneous liquid. —*Bull. de l'Acad. Roy. de Belgique*, S. 2. vol. xxiii. No. 3: 1872.

THE  
LONDON, EDINBURGH, AND DUBLIN  
PHILOSOPHICAL MAGAZINE  
AND  
JOURNAL OF SCIENCE.

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[FOURTH SERIES.]

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JUNE 1872.

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LI. *A new discussion of the Hydrodynamical Theory of Magnetism.* By Professor CHALLIS, M.A., LL.D., F.R.S.\*

A THEORY of Magnetism founded on propositions in Hydrodynamics was originally proposed by me in the Numbers of the Philosophical Magazine for January and February 1861. The same theory, considerably modified, is given in my work 'On the Principles of Mathematics and Physics;' and the Number of the Philosophical Magazine for July 1869 contains an article the purpose of which is to make the mathematical part of the theory more complete. In the present communication I propose to discuss anew the principles on which magnetic phenomena are explained according to this theory, with the view of correcting or extending the results previously obtained. The discussion will necessarily involve to a considerable extent the hydrodynamical theory of *galvanic currents*.

1. I assume, as I have already done, (1) that all the *active* forces in nature are different modes of pressure, under different circumstances, of a universal elastic æther, which may be mathematically treated as a continuous substance pressing always proportionally to its density; (2) that all visible and tangible bodies consist of inert spherical atoms of constant magnitude, held, when undisturbed, in positions of equilibrium by attractive and repulsive forces, the laws of which result both from the active pressure of the æther and the passive resistance of the atoms due to the constancy of their form and magnitude. The æther at rest is accordingly assumed to be everywhere of the same den-

\* Communicated by the Author.

*Phil. Mag.* S. 4. Vol. 43. No. 288. June 1872.

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sity; and it is, further, supposed that the atoms are so small that even in dense bodies the space which they occupy is very small compared with the intervening spaces.

2. In general, when the atoms of any substance are in positions of stable equilibrium, the attractive forces acting on any atom in the direction of any straight line drawn through its centre will just counteract each other; and the same will be the case with respect to the repulsive forces. But it is also conceivable that the equilibrium of the atom may be maintained by the mutual counteraction of attractive and repulsive forces. For instance, let the substance be a long rectangular bar of steel, slightly increasing in density by regular gradations from one end to the other, and of uniform density in any transverse section. Then the direction of the resultant of the molecular attractive forces will be parallel to the length of the bar, as well as that of the resultant of the atomic repulsive forces; and it is therefore supposable that by the former any atom may be as much attracted towards the denser end as by the latter it is repelled towards the rarer end. This, in fact, is assumed in the present theory to be the case in a permanently magnetized bar of steel, the gradation of density being conceived to be originally generated by the usual processes of magnetization, and to be maintained exclusively by the proper atomic and molecular forces of the steel. In soft iron a like gradation of density, produced by the action of an extraneous magnet, or that of a galvanic current, continues only so long as the action lasts. In diamagnetic substances, such as bismuth, gradation of density is similarly produced and maintained by the action of a magnet, but the direction in which the density increases is opposite to that in soft iron under analogous circumstances. Also the application of *heat* under certain conditions gives rise to magnetic or galvanic phenomena, which, according to this theory, are referable to the production of gradations of atomic density by the dynamical action of the heat.

3. In all cases of the existence of such gradation of density, it can be shown on the above-stated hypotheses, by reasoning which will be presently indicated, that motions either of the æther within the body, or of the body relative to the æther, have the effect of producing *accelerations* of the æther. The *secondary* currents thus generated are considered to be the immediate cause of magnetic or galvanic attractions and repulsions. Also, in the Theory of Frictional Electricity which I proposed in the Philosophical Magazine for October 1860, electrical attractions and repulsions are accounted for in the same manner. Hence as the generation of secondary streams under the above-stated conditions is a fundamental proposition in the hydrodynamical theories



of these three physical forces, I shall now endeavour to give as exact a proof of it as may be possible. The mathematical reasoning relating thereto under the head of the Theory of Electric Force in pages 545-547 of my work 'On the Principles of Physies' is incomplete and inaccurate.

4. Suppose a current of the æther to traverse a substance consisting of atoms so arranged that their number in a given space increases regularly, but by very small gradations, in a given direction, and conceive the whole of the space occupied by atoms to be very small compared with the intervening space. Also, the motion being (at first) assumed to be steady, let  $V$  be the average velocity, and  $\rho$  the average density of the æther at a certain position  $A$ , and let  $B$  be another position distant by  $\delta z$  from  $A$  in the direction in which the atomic density *increases*. Then if  $D$  be the proportion of the space occupied by atoms to the whole of the given space, or, as it may be called, *the contraction of channel*, the quantity of fluid which passes a unit of area at  $A$  in the unit of time is  $V\rho(1-D)$ . But it has been proved (see Phil. Mag. for June 1864, p. 458) that when a stream is incident on a small sphere, the mean flow in the original direction is not altered by the disturbance of the lines of motion caused by the reaction of the sphere; and the same is the case if there be many such spheres, provided the contraction of channel they produce is very small. Hence, supposing the initial generation of the stream to have been such that the same quantity of fluid was made to pass each element of a given transverse section in a given small interval, according to the above reasoning this condition will be fulfilled at any subsequent epoch at each transverse section, notwithstanding the presence of the atoms; so that the quantity of fluid which passes through an elementary space at  $B$  will be the same in the same interval as that which passes through an equal space at  $A$ . Consequently

$$\frac{\delta \cdot V\rho(1-D)}{\delta z} = 0.$$

Hence, passing from small differences to differentials, the reasoning being independent of the magnitude of  $\delta z$ , we have

$$\frac{d\rho}{\rho dz} - \frac{dD}{(1-D)dz} + \frac{dV}{Vdz} = 0. \quad . \quad . \quad . \quad (a)$$

Now, the motion being steady, the fluid unlimited in extent, and no extraneous force acting, if  $\rho_0$  be the value of  $\rho$  where  $V=0$ , the equation  $\rho = \rho_0 e^{\frac{V^2}{2a^2}}$  is applicable at all points, even when the effect of the reaction of the atoms is taken into account. Con-

sequently, by employing this equation to eliminate  $\frac{dV}{dz}$  from (a), it will be found that

$$-\frac{a^2 d\rho}{\rho dz} = \frac{a^2 V^2}{a^2 - V^2} \cdot \frac{dD}{(1-D)dz} = \frac{V^2 dD}{(1-D)dz}, \text{ nearly.} \quad (b)$$

The force on the left-hand side of this equation, which is entirely due to the gradation of atomic density expressed by  $\frac{dD}{dz}$ , takes effect in producing the streams I have called *secondary*; and the reasoning shows that the intensity of such streams is independent of the *direction* of the primary current. (See the Theory of Electricity in the Philosophical Magazine for October 1860, art. 18).

5. In the applications of the above views respecting the generation of secondary streams to the Theories of Electricity, Galvanism, and Magnetism, I have assumed (Principles of Physics, pp. 547 & 548) that the *primary* stream, the velocity of which is  $V$ , might be one which relatively passes through all terrestrial substances in consequence of the earth's motion about the sun, and that two other primary streams would similarly be due to the earth's rotation about its axis, and the motion of the solar system in space. It was also argued that the resulting secondary stream would be the *sum* of those which the three primaries, supposed to be steady motions, would produce separately, and that it would consequently be *quam proxime* steady.

But it has since appeared to me that this argument cannot be maintained, and that the generation of the secondary stream must be ascribed to the *resultant* at each instant of the three primary velocities. Since there is reason to conclude that the motion of the solar system is comparable with the earth's orbital motion, that resultant would be subject to large variations, to which there is nothing corresponding in the observed intensity of magnetism. Hence the movements of the earth fail to account for the primary velocity of the theory.

6. The formula (b) shows that the force which generates the secondary streams varies as the square of the primary velocity  $V$ , since the factor  $\frac{dD}{(1-D)dz}$  may be considered to be constant. Now,

since no observed magnetic variations are attributable to variations of the primary velocity such as those which the composition of the earth's velocity with that of the solar system might be supposed to produce, *à fortiori* the much smaller variations of the earth's velocity in its orbit can have no perceptible effect. This inference agrees with a conclusion drawn by the Astronomer Royal from discussions of the Greenwich Magnetical Observa-

tions made in 1848–1857, and in 1858–1863. In the volume for 1867, p. cciv, he says, “We are justified in stating that there is no certain evidence for Annual Inequality.” The same result is arrived at with respect to the Horizontal Force. The annual inequalities deduced from the theory in ‘The Principles of Physics’ (pp. 657–660) were obtained by employing the argument which is shown above to be untenable.

It is true, however, that General Sir E. Sabine has deduced very small annual inequalities of Dip, Total Force, and Declination by discussions of observations taken at Kew, Toronto, Hobarton, St. Helena, and the Cape of Good Hope (see Phil. Trans. for 1863, p. 307). But it is possible that these may be due to a cause, distinct from the earth’s motions, which is adverted to in art. 36 of the communication on magnetic force in the Philosophical Magazine for February 1861, and will be more fully treated of in the sequel of the present communication.

7. It remains, therefore, to determine by what means the streams to which the theory ascribes the magnetism of a steel magnet are generated, the magnet being either a straight bar or in the form of a horseshoe. This question I shall endeavour to answer by taking account of the theories of *atomic repulsion* and *molecular attraction* proposed in the Numbers of the Philosophical Magazine for March and November 1859 and February 1860, and in ‘The Principles of Physics’ (pp. 459–464). For this purpose the following general hydrodynamical theorem, which, as far as I am aware, has not hitherto been recognized, will be made use of:—Whenever the lines of motion in a given fluid element are normals to a continuous surface, so that the element is *changing form* by reason of the motion, the function  $u dx + v dy + w dz$  is an exact differential. In proof of this theorem it seems sufficient to say that the change of form of a given element in consequence of convergency or divergency of the lines of motion is a distinctive property of a fluid, whereby its motion is separated from that of a solid, and that the integrability of that differential function is the sole and necessary analytical expression of this property.

8. This being understood, we have, as is known, for a fluid, defined by the relation between the pressure and density expressed by  $p = a^2 \rho$ , the general equation

$$a'^2 \text{ Nap. log } \rho + \frac{d\phi}{dt} + \frac{V^2}{2} + f(t) = 0,$$

where  $a'$  is put for  $\kappa a$  ( $\kappa$  being a known numerical factor, the theoretical determination of which I have discussed in previous communications), and  $(d\phi) = u dx + v dy + w dz$ . No extraneous force being supposed to act, this equation is applicable to all



points of the fluid at all times; and if the fluid be disturbed within a limited space and be of unlimited extent, there must be distant points at which  $\frac{d\phi}{dt}$ , or  $\int \left( \frac{du}{dt} dx + \frac{dv}{dt} dy + \frac{dw}{dt} dz \right)$ , vanishes together with  $V$ , and the density has a constant value  $\rho_0$ . Hence

$$\rho = \rho_0 e^{-\frac{d\phi}{a^2 dt} - \frac{V^2}{2a^2}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (c)$$

This equation applies generally to *unsteady* motion. Now it is to be observed that, whether the motion be steady or unsteady, the investigation of the equation (a) is the same, because in both cases the mean quantity of fluid which passes a given transverse area in a given time is not sensibly altered by the reaction of one or more small spheres. Hence, eliminating  $\frac{dV}{dz}$  from (a) by means of (c), the result is

$$-\frac{a^2 d\rho}{\rho dz} = \frac{V^2 dD}{(1-D)dz} + \frac{d^2\phi}{dz dt} \text{ very nearly, } . \quad . \quad (d)$$

which equation differs from that for steady motion by having an additional term on the right-hand side. If this equation be applied in a case in which  $V$  represents the velocity in *vibratory* motion, the additional term will have as much positive as negative value, so that the mean effect of the impulses it indicates will be zero. The other term is indicative of impulses towards the denser part of the substance, whether  $V$  be positive or negative, just as when the motion is steady.

9. Recurring now to the before-mentioned theories of atomic repulsion and molecular attraction, according to which the repulsion which keeps the atoms asunder is due to vibrations emanating from individual atoms, while the counteracting attractions result from composite vibrations emanating from a congeries of atoms constituting a molecule, it will appear that the maximum velocity and breadth must be supposed to be much greater in the latter vibrations than in the others. Also it is presumable that the maximum velocity may very much exceed the velocity of the earth in its orbit, or that of the solar system in space, and yet be small compared with the value of  $a'$ , which is about 190,000 miles per second. In the supposed case of the atoms being constrained to take positions such as to produce a regular gradation of atomic density from one end to the other of a steel bar, attraction-vibrations, propagated in the direction from the denser to the rarer end, will continually counteract the atomic repulsions urging the atoms towards the rarer end, without being neutralized by attraction-vibrations of the same order propagated in

the opposite direction. To the velocity ( $V$ ) in these outstanding vibrations it is reasonable to attribute the generation, in the manner explained above, of the magnetic streams of the theory. In fact the generation of such streams may be regarded as a reaction arising from the state of constraint into which the substance is put by the abnormal relative positions of its atoms. According to these views the magnetism of a steel bar is in no sensible degree due to the earth's motions relative to the æther, but results from vibratory motions of the æther of the order of those by which, in previous researches, I have endeavoured to account theoretically for intrinsic molecular forces. This is an important correction of the principles I have hitherto adopted in the hydrodynamical theory of magnetism.

10. It is a general law of magnetic streams that they are *re-entering*. The streams, for instance, which issue from that which is assumed to be the denser end of a magnet are turned back, and, after flowing in the direction of the magnet's length, enter it at the other end. This general law admits of being accounted for theoretically as follows. It is evident that the acceleration of a mass of unlimited dimensions by the action of a finite pressure on a finite surface is an infinitesimal quantity of the *third* order, and that, consequently, if the mass be a fluid as nearly incompressible as the æther is assumed to be, such pressure produces absolutely no movement of the *whole* mass in either a finite or an infinite interval of time. Hence the displacement, by the pressure, of any portion of the fluid in the direction of its action must immediately give rise to the displacement of an equal portion in the contrary direction. In other words, there can be no permanent flow of the fluid across any plane perpendicular to the direction of the impulses. To satisfy this condition the motion must take place in reentering courses or circuits. Thus the existence of *complete circuits* as a necessary condition of magnetic, as also of galvanic phenomena, is accounted for by the hydrodynamical theory. I am not aware that any other *à priori* explanation of this very general and prominent characteristic of physical currents has been given.

11. Another general law relating to galvanic and magnetic circuits may be referred to the hydrodynamical fact that the currents of a fluid always take the *easiest* course—that is, the course in which the least resistance from the inertia of the fluid is to be overcome. In a bar magnet there is very little magnetism about its middle part in directions transverse to its length, the magnetic action taking place chiefly about the two ends, as is shown by immersing the magnet in iron-filings. The hydrodynamical explanation of these facts is as follows. About the middle of the magnet there is no transverse impulse capable of

overcoming in any sensible degree the inertia of the circumjacent fluid; while the impulses in the direction of the increase of density have the effect of causing a stream to flow out of the parts near the denser end in courses which, by reason of the inertia of the fluid beyond, are at first made divergent, and eventually are turned completely backward: these return-currents are then opposed by the inertia of the fluid beyond the other end of the magnet, and are made to converge towards that end in such manner as to fulfil by the easiest courses the necessary condition of motion in circuits. According to this view the courses are determined by a law of least action. (See a mathematical theory of this kind of motion in the *Philosophical Magazine* for July 1869.)

12. Like considerations are applicable if the magnet has a form different from that of a straight bar; for instance, if it has the form of a horseshoe. In this case, as the two ends are brought near each other, the mean course of least resistance is along the axis of the magnet, and the stream passes out of one end immediately into the other. Also the tendency of the stream to escape from the curved part of the magnet by reason of centrifugal force is still opposed by the inertia of the external fluid. In order to make the insulation of the current more complete, that part is usually covered with sealing-wax, this substance not having the property of easily conducting ætherial streams.

The same hydrodynamical principles account for the flow of a *galvanic* current through conducting substances of very irregular forms. In consequence of the resistance to emergence arising from the inertia of the surrounding æther, the currents are confined within the boundaries of the conductors just as a stream of water is confined within channels, such as rigid pipes, or vessels of any form through which it is compelled to flow. Only, in the case of the galvanic current, the condition of a complete circuit is required to be fulfilled in order that the flowing may take place.

13. It might be urged as an objection to the foregoing reasoning, that if an unlimited mass of elastic fluid, such as the æther is conceived to be, were to receive, within certain limits of distance from a centre, impulses directed from the centre and equal in all directions, the effect of these impulses would not be neutralized by reaction from the inertia of the surrounding fluid. This objection admits of being answered by an appeal to hydrodynamical principles which I proposed long since, and have repeatedly insisted upon, although they have not hitherto received general recognition. I have pointed out that when an elastic fluid receives impulses equally in all directions from a centre,



either at a given distance during a limited interval of time, or at a given instant through a limited space, the subsequent motion cannot be a solitary wave of condensation or of rarefaction; for in such case the condensation would vary inversely as the *square* of the distance from the centre, whereas the mathematical solution of the problem shows that it varies simply as the inverse of the distance. To meet this difficulty I now adhere to the arguments I adduced in an article in the *Philosophical Magazine* for January 1859, and in paragraph 10 of an article in the *Philosophical Magazine* for June 1862, although subsequently I adopted a different view. According to those arguments the law of the simple inverse of the distance holds good only in case the disturbance gives rise both to condensation and rarefaction and the resulting motion is consequently *vibratory*. It must therefore be admitted that, whether the original impulses are vibratory or not, alternations of condensation and rarefaction are actually produced; and it seems evident that this effect must be attributed to the obstacle opposed to the impulsive action by the inertia of the surrounding mass of fluid. This explanation is, I think, complete when it is supplemented by the consideration that, according to the hydrodynamical principles above referred to, vibratory motion, accompanied by alternate condensation and rarefaction, may be shown to be proper to an elastic fluid antecedently to any suppositions respecting particular modes of disturbance. (See the demonstration of Prop. X. in the *Philosophical Magazine* for December 1854, and that of Prop. XI. in the 'Principles of Mathematics,' pp. 201-205.)

14. Again, when the motion of an elastic fluid is supposed to be in directions perpendicular to a given plane, the usual process for determining the velocity and condensation at any point conducts, as I long since remarked, to a contradictory result, indicative of faulty reasoning. (See 'Principles of Mathematics,' pp. 193-195.) As in the foregoing case of central motion, the contradiction is significant of an effect of the inertia of the fluid not taken into account by that process. By first proving, antecedently to the consideration of arbitrary modes of disturbance, that the fluid is susceptible, by reason of its inertia, of spontaneous vibratory motions partly parallel and partly transverse to an axis, and thence arguing that arbitrarily impressed motions must be regarded as actually composed of such primary motions, I have shown that the above-mentioned contradiction disappears. (See Prop. XI. above cited.) The conclusions arrived at in this and the preceding paragraph respecting the generation of vibratory motions by impulses that are not vibratory, are of essential importance in accounting for a large class of phenomena of light on the hypothesis of undulations. Also

certain effects of motions of the air—for instance, the sounds of recognizable pitch resulting from the mutual collision of aerial streams, or from the diversion given to such streams by encountering solid obstacles, are explainable on the same principles.

15. I take occasion here to remark that the generation and propagation at the surface of water of a series of circular waves in forms which appear to be independent of the mode of disturbance, or shape of the disturbing body, are, I think, referable to dynamical reasons analogous to those adduced above. Also the series of small waves which are seen to precede a cylindrical rod when it is held vertically and moved horizontally through water in which it is partly dipped, may be similarly accounted for. (These “ripples,” together with the broader waves which follow the rod under the same circumstances, are described and discussed by Professor W. Thomson in an article in the *Philosophical Magazine* for November 1871.) Supposing the fluid to be one of perfect fluidity, the foregoing argument, which is based on that supposition, leads to the conclusion that the generation of the ripples may be ascribed to the obstacle opposed to the motion of the rod by the inert mass of fluid in front, and that the waves behind are broader than those before by reason of the *reluctance* with which the mass behind, on account of its inertia, follows the rod.

16. The motions which have been thus far considered are all such that each element of the fluid is at each instant changing its form, and the lines of motion are normals to continuous surfaces, so that  $udx + vdy + wdz$  is always and everywhere an exact differential. This may be true even supposing the motion to be in directions perpendicular to a given plane, because, as I have indicated above, the rectilinear motion may be composite, in which case the change of form of the fluid elements takes place with respect to each of the component motions. There are, however, cases of the motion of a fluid in which each element maintains always the same form, either because the whole mass moves or rotates as if it were solid, or consists of an unlimited number of parts which individually so move. Such motions are distinguished by the analytical circumstance that for them  $udx + vdy + wdz$  is integrable by a *factor*. To prove this is the object of the following argument, in which, for the sake of brevity, the fluid is supposed to be incompressible.

17. For proving a proposition of this kind it is necessary to employ the general equations of hydrodynamics, in order that the reasoning may depend on the fundamental principles which these equations express. I shall therefore begin by drawing an inference from that which I call the equation of continuity, namely:—

$$\frac{d\psi}{dt} + \lambda \left( \frac{d\psi^2}{dx^2} + \frac{d\psi^2}{dy^2} + \frac{d\psi^2}{dz^2} \right) = 0, \quad . \quad . \quad . \quad (e)$$

which takes account only of space, time, and motion. Since  $u = \lambda \frac{d\psi}{dx}$ ,  $v = \lambda \frac{d\psi}{dy}$ ,  $w = \lambda \frac{d\psi}{dz}$ , it follows that the left-hand side of this equation is the complete differential coefficient of  $\psi$  with respect to the coordinates and the time; so that  $\left(\frac{d\psi}{dt}\right) = 0$ , and by integration  $\psi = C$ , an arbitrary quantity not containing  $t$ . Hence, since  $\psi$  does not change with the time, the equation  $\psi - C = 0$  shows that each surface of displacement maintains an invariable position. Now there are only *two* ways in which this condition can be fulfilled when the forms of the elements are also invariable; either the motion is in straight lines perpendicular to a fixed plane, or in circles about a fixed axis.

18. First, let the motion be in directions perpendicular to a plane, which we will suppose to be the plane of  $xy$ , and let the velocity along any line the coordinates of which are  $x$  and  $y$  be  $f(x, y)$ . Then we have  $u = 0$ ,  $v = 0$ ,  $w = f(x, y)$ ; so that  $udx + vdy + wdz$  becomes  $f(x, y)dz$ , which is not integrable *per se*, but plainly may be made integrable by the factor  $\frac{1}{f(x, y)}$ . Then

$$(d\psi) = \frac{w}{f(x, y)} dz = dz; \text{ and by integrating, } \psi = z + \chi(t). \text{ But}$$

it is shown above that  $\psi$  is independent of  $t$ ; so that  $\chi(t) = 0$ . Hence, since  $\psi$  is equal to a constant  $C$ ,  $z - C = 0$  is the general equation of the surfaces of displacement, which, accordingly, are planes perpendicular to the axis of  $z$ . Also the motion will be the same at all points of a given filament of the fluid parallel to the axis of  $z$ ; but, since  $w = f(x, y)$ , it may be supposed to vary from one filament to another. The proposition is thus proved for this case, the result having been obtained by means of a factor.

19. Next let the motion be in circles about the axis of  $z$ . Then,  $V$  being the velocity at the distance  $r$  from the axis, we have at the point  $xyz$

$$u = -\frac{Vy}{r}, \quad v = \frac{Vx}{r}, \quad w = 0,$$

and  $udx + vdy + wdz = -\frac{V}{r} (ydx - xdy)$ , which is not an exact differential. It is evident that the factor  $\frac{1}{Vr}$  will make it such,



and we shall thus have

$$(d\psi) = \frac{xdy - ydx}{x^2 + y^2} = d \cdot \tan^{-1} \frac{y}{x}.$$

Hence by integrating,  $\psi = \tan^{-1} \frac{y}{x}$ , no arbitrary function of  $t$  being added, because it has already been shown that  $\psi$  is equal to a constant  $C$  which is independent of the time. Consequently  $C = \tan^{-1} \frac{y}{x}$ , or  $y = x \tan C$ ,  $C$  being an arbitrary arc. This

general equation of the surfaces of displacement indicates that the motion is in circles about the axis of  $z$ . This result having been arrived at by means of a factor, the proposition that  $udx + vdy + wdz$  is integrable by a factor for this kind of motion is thereby demonstrated.

20. It is now to be observed that although the general equation (e) is satisfied by the two supposed kinds of motion, the possibility of such motions is not *proved* till the other general equations have been taken into account. Yet, according to the essential principles of applied calculation, the circumstance that that general equation has been satisfied cannot be without significance; and it is on this account necessary to inquire whether and under what conditions the other general equations are satisfied by the same motions.

21. Taking, first, the motion in parallel straight lines, since  $u=0$ ,  $v=0$ , and  $w=f(x, y)$ , it is evident that the general equation of constancy of mass,

$$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0, \quad . \quad . \quad . \quad . \quad . \quad (f)$$

is at once satisfied, and it only remains to take account of the dynamical equations

$$\frac{dp}{dx} + \left(\frac{du}{dt}\right) = 0, \quad \frac{dp}{dy} + \left(\frac{dv}{dt}\right) = 0, \quad \frac{dp}{dz} + \left(\frac{dw}{dt}\right) = 0. \quad . \quad (g)$$

By substituting the values of  $u$ ,  $v$ , and  $w$  in these equations there will result

$$\frac{dp}{dz} = 0, \quad \frac{dp}{dy} = 0, \quad \frac{dp}{dx} = 0;$$

whence it follows that  $(dp) = 0$ , and  $p$  is constant. Since the last of the three equations is equivalent to

$$\frac{dp}{dz} + \frac{dw}{dt} + u \frac{dw}{dx} + v \frac{dw}{dy} + w \frac{dw}{dz} = 0,$$

it may be remarked that the foregoing reasoning, since  $u=0$

and  $v=0$ , does not exclude finite values of  $\frac{dw}{dx}$  and  $\frac{dw}{dy}$ , and consequently it is possible that  $w$  may vary from one line of motion to a contiguous one. Thus it has been shown that the supposed motion in parallel lines satisfies all the general equations (e), (f), and (g).

22. Proceeding, now, to the case of rotatory motion about the axis of  $z$ , it will be found, on substituting in the equation (f) for  $u, v, w$  the respective values  $-\frac{Vy}{r}, \frac{Vx}{r}, 0$ , that the result is

$$-\frac{y}{r} \frac{dV}{dx} + \frac{x}{r} \cdot \frac{dV}{dy} = 0.$$

This is a partial differential equation, the solution of which by the usual process is  $V=F(r)$ . It is thus proved that the circular motion is a function of the distance from the axis and of arbitrary value. It remains to ascertain under what dynamical conditions this kind of motion is possible.

23. Since  $w=0$ , the equations to be used for this purpose are

$$\left. \begin{aligned} \frac{dp}{dx} + \frac{du}{dt} + u \frac{du}{dx} + v \frac{du}{dy} &= 0, \\ \frac{dp}{dy} + \frac{dv}{dt} + u \frac{dv}{dx} + v \frac{dv}{dy} &= 0. \end{aligned} \right\} \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (h)$$

By substituting in these equations the foregoing values of  $u$  and  $v$ , it will be found that

$$(dp) = \frac{V^2}{r} dr - r \frac{dV}{dt} d \cdot \tan^{-1} \frac{y}{x}.$$

In order that the right-hand side of this equation may be an exact differential, we must have  $\frac{dV}{dt} = 0$ ; so that  $V$  is a function of  $r$  without containing  $t$ , and the motion is thus shown to be steady. Hence also

$$\frac{(dp)}{dr} = \frac{V^2}{r};$$

that is, the centrifugal force is counteracted by variation of pressure with the distance. Since the right-hand side of this equation is necessarily positive, the pressure  $p$  continually increases with the distance.

24. Suppose, in consequence of what has now been proved, that for the case of motion in parallel straight lines we have  $\psi_1 = C_1$ , and for the circular motion  $\psi_2 = C_2$ , and that the sum of the two equations gives  $\psi = C$ . Since this composite equation is of the same form as the components, it follows that, so far as

the principle of continuity is concerned, the two motions might *coexist*. It may also be readily shown that this coexistence is compatible with the equation of constancy of mass. For if  $u_1, v_1, w_1$  and  $u_2, v_2, w_2$  represent respectively the components of the velocity for the two motions, and if  $u = u_1 + u_2, v = v_1 + v_2, w = w_1 + w_2$ , we have, as already proved,

$$\frac{du_1}{dx} + \frac{dv_1}{dy} + \frac{dw_1}{dz} = 0, \quad \frac{du_2}{dx} + \frac{dv_2}{dy} + \frac{dw_2}{dz} = 0,$$

and, by adding the two equations,

$$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0.$$

Having by this result proved that the component motion fulfils the condition of constancy of mass, we have, further, to inquire whether  $u dx + v dy + w dz$  can be made integrable by a factor; and if so, by what factor. For this purpose we have

$$u = u_1 + u_2 = -\frac{Vy}{r}, \quad v = v_1 + v_2 = \frac{Vx}{r}, \quad w = w_1 + w_2 = f(x, y),$$

which values of  $u, v$ , and  $w$  evidently show that  $u dx + v dy + w dz$  is not an exact differential. In order that it may be made integrable by a factor, the following equation of condition, as is known, must be satisfied:

$$u \left( \frac{dv}{dz} - \frac{dw}{dy} \right) + v \left( \frac{dw}{dx} - \frac{du}{dz} \right) + w \left( \frac{du}{dy} - \frac{dv}{dx} \right) = 0.$$

Putting, for shortness,  $f$  for  $f(x, y)$ , and for  $V$  its value  $F(r)$ , the result of substituting the values of  $u, v, w$  will be found to be

$$\frac{y}{f} \frac{df}{dy} + \frac{x}{f} \frac{df}{dx} = 1 + \frac{rF'(r)}{F(r)}.$$

This equation makes it evident that the equation of condition cannot be satisfied unless  $f$  be a function of  $r$ . Putting therefore  $f(r)$  for  $f$ , we have

$$\frac{rf'(r)}{f(r)} = 1 + \frac{rF'(r)}{F(r)},$$

and by integration,

$$\frac{f(r)}{F(r)} = \frac{r}{b}, \quad \dots \dots \dots (i)$$

$b$  being an arbitrary constant. This result establishes a relation between  $f(r)$  and  $F(r)$ , by taking account of which it will be seen that

$$u dx + v dy + w dz = rF(r) \left( \frac{x dy - y dx}{r^2} + \frac{dz}{b} \right).$$



Hence the required factor is  $\frac{1}{rF(r)}$ , and

$$(d\psi) = \frac{x}{r^2} dy - \frac{y}{r^2} dx + \frac{1}{b} dz.$$

Thus the differential equation of any surface of displacement is

$$dz = -b \cdot \frac{xdy - ydx}{r^2},$$

and by integration the equation of the surface becomes

$$z = c - b \tan^{-1} \frac{y}{x}. \quad . \quad . \quad . \quad . \quad . \quad (k)$$

These results give the means of defining exactly the character of the motion. According to the principle of easy divisibility, we may conceive the fluid to be divided into an unlimited number of infinitely thin cylindrical shells having the axis of  $z$  for their common axis; and if  $F(r)$  be the rotatory velocity of a given shell at the distance  $r$  from the axis, then will  $f(r)$ , or  $\frac{rF(r)}{b}$ , be the velocity of the same shell parallel to the axis. Con-

sequently the motion of any given point of the shell will be in a *spiral*. The form and position of the spiral may be inferred from the above equation of the surfaces of displacement when the values of the constants  $b$  and  $c$  are given. The spiral is left-handed or right-handed according to the sign of  $b$ .

25. We have still to determine the dynamical conditions of this motion. From what has been proved, the components of the velocity have the following values:

$$u = -\frac{y}{r} F(r), \quad v = \frac{x}{r} F(r), \quad w = f(r) = \frac{r}{b} F(r);$$

and for determining the pressure, since  $u$ ,  $v$ ,  $w$  are independent of  $z$ , we have

$$\frac{dp}{dx} + u \frac{du}{dx} + v \frac{du}{dy} = 0, \quad \frac{dp}{dy} + u \frac{dv}{dx} + v \frac{dv}{dy} = 0, \quad \frac{dp}{dz} + u \frac{dw}{dx} + v \frac{dw}{dy} = 0.$$

The last of these equations gives

$$\frac{dp}{dz} = -\frac{xy}{r^2} F(r)f'(r) + \frac{xy}{r^2} F(r)f'(r) = 0.$$

Hence the equations for determining  $p$  are the same as for simple rotation about the axis, and, just as for that case,  $\frac{(dp)}{dr} = \frac{V^2}{r}$ , or the centrifugal force is counteracted by the increment of the pressure with the distance from the axis.

(The foregoing investigation takes account of all the cases of

motion for which  $udx + vdy + wdz$  is integrable by a factor. When that differential is exact, the general equation (e) conducts to a unique result belonging to a different class of motions, as I have elsewhere shown: *Principles of Mathematics*, pp. 186–188. The equation is in that case satisfied by *rectilinear* motion, which, by reason of the other general equations, is restricted to motion along an axis of longitudinal and transverse vibrations. Motions of the vibratory class are inapplicable to galvanic and magnetic phenomena.)

26. The mathematical investigation concluded above gives the means of explaining in what manner a *galvanic current* flows to any distance along a fine cylindrical wire of copper. Together with the movement of a stream along the wire, both within it and outside, and symmetrical with respect to its axis, there must be transverse circular motion about the axis; for otherwise, since by the contraction of channel the velocity is greater, and the pressure less, within the wire than in the circumjacent space, the fluid would flow from all sides towards the axis, and thus a stop would be put to the current. The spiral motion which results from the composition of the longitudinal and transverse motions, being accompanied by centrifugal force due to the circular motion, has the effect of maintaining the current. When the continuity of the wire, or of any other substance by which a current is conducted, is abruptly broken, at the first instant the stream issues from the terminal, and impinges on the surrounding fluid; but since it ceases at the same instant to be maintained by means of circular motion about an axis, the compound motion is immediately converted into the kind for which  $udx + vdy + wdz$  is an exact differential, and is consequently turned back by having to encounter the inertia of an unlimited mass of the fluid. The return course will be along the original conductor if this be the only, or the readiest, channel. If, however, another wire conductor should be in the neighbourhood of the first, there would seem to be no reason why the revulsion due to the fluid's inertia should not cause a partial return of the fluid along this wire also. In fact it has been proved experimentally by Faraday that such motion actually takes place. I venture here to express the opinion that no explanation of this induction of a galvanic current, other than one resting on hydrodynamical principles, is likely ever to be discovered.

27. The inquiry as to the *origination* of the rectilinear and circular motions which combine to produce a galvanic current is distinct from the preceding investigation, inasmuch as it involves, together with deductions from hydrodynamical principles, considerations respecting the chemical action between dissimilar bodies, as also respecting the effect of the particular arrange-

ment of the atoms or molecules of the conducting substance. On these points it will now be proper to make some remarks.

Galvanic currents may be conceived to be generated in the same manner as magnetic, so far as regards the condition of a gradation of atomic density, the gradation being in their case produced and maintained by chemical action in such manner as to exist permanently in the neighbourhood of the surfaces of contact of the substances between which the action takes place. The currents thus generated must, from what has already been argued, fulfil the condition of flowing in a complete circuit in order that they may be permanent; for which reason it is necessary to connect the poles of a galvanic battery by some material (as copper wire) capable, in respect both to form and quality, of conducting galvanic currents. But from the foregoing mathematical argument it may be inferred that currents so conducted can proceed only in *spiral* courses. Now, supposing the course to be of this kind, it appears from experiment that the *turn* of the spiral is always in the *same* direction relative to the course of the current along the conductor. Hence it would seem that such courses are impressed by the wire itself or other conducting substance, because, as far as regards hydrodynamical conditions, the course might either be *dextrorsum* or *sinistrorsum*. Supposing the mean direction of the current to be in the positive direction of the ordinates  $z$ , if the arbitrary constant  $b$  in the foregoing equation ( $k$ ) be positive, the value of  $z$  is greater as the  $\arctan^{-1} \frac{y}{x}$  is less, and therefore the spiral course is *dextrorsum*; that is, to a person looking along the axis in the positive direction the turn of the spiral above the axis is from the left hand to the right. But hydrodynamically it is equally possible that  $b$  may be negative, in which case  $z$  is greater as  $\arctan^{-1} \frac{y}{x}$  is greater, and the turn of the spiral is *sinistrorsum*.

28. Assuming, for the reasons above given, that the direction of the spiral course is determined by the atomic or molecular constitution of the conductor, it is conceivable that this effect may be attributable to a particular *arrangement* of the constituent atoms, causing the path of least resistance, instead of being rectilinear, to be continuously maintained in a spiral form. For instance, such a modification of the path might be produced if the mean retardation, due both to the reaction and the arrangement of the atoms, operated in a direction not exactly opposed to that of the stream.

29. The quantity of fluid which in a unit of time passes a plane perpendicular to the axis of the wire is proportional to the integral of  $f(r)dr$  between certain limits, and is therefore greater



as  $f(r)$  is greater; and since  $bf(r) = rF(r)$ , for a given value of  $r$   $f(r)$  is greater as the circular motion  $F(r)$  is greater. The latter function may be considered to be the exponent of the capacity of the substance for generating spiral motion, and thereby conducting galvanic currents. This property, which exists in very different degrees in different substances, seems to be possessed in an eminent degree by *copper*, wires of this metal being generally used for conducting galvanic currents. I shall have occasion to advert again to the origination of spiral motion by the agency of copper.

30. The mathematical theory being incapable of determining by itself whether the spiral motion is dextrorsum or sinistrorsum, it remains to inquire how far this question may be decided by combining experiment with the theory. Experiment, in the first place, may be taken to indicate that the direction of the theoretical circular motion about a rheophore is related in a constant manner to the direction of the current along the rheophore, so that when the latter is given the other should admit of being inferred. In order to determine that relation I make the provisional hypothesis that currents flow, or tend to flow, *out of* the poles which experimenters call *positive*, and *into* those which they call *negative*. That pole of a horizontal magnetic needle which is southward is called the positive pole. Hence, by the hypothesis, magnetic streams flow out of it, and enter into the pole which is northward; whence it follows, by the hydrodynamical theory of the magnetic needle (see *infra*, art. 33), that the horizontal components of the terrestrial streams flow *southward* both in the northern and southern magnetic hemispheres, and by consequence, as the dipping-needle shows, that the vertical components flow *upwards*. Thus the total terrestrial streams issue from the earth at and about the north magnetic pole, and enter into it at and about the south magnetic pole.

31. Again, that pole of a voltaic pile or galvanic battery which has the *zinc* terminal plate is called *positive*, and that which has the *copper* terminal plate *negative*. Hence, by the hypothesis, the galvanic current along the connecting rheophore outside the pile or battery flows *from the zinc to the copper*.

[I have previously taken the current to be in the contrary direction, having had difficulty in deciding between the discordant indications of experimenters on this point. The direction shown by arrows and the signs + and - in the figure in art. 682 of Atkinson's edition of 'Ganot' does not accord with the present hypothesis; neither, in fact, is it consistent with directions similarly indicated in other cases in the same work.]

32. By referring to two experiments described in arts. 732 and 734 of Atkinson's 'Ganot,' it will be seen that the coexist-

ence of the horizontal terrestrial current with a vertical galvanic current produces an action on the rheophore tending *westward* or *eastward* according as the vertical current is *ascending* or *descending*. Now on the hypotheses that the earth's current is southward, and the galvanic current in the direction from the zinc terminal to the copper terminal, these facts are explainable only on the supposition that the spiral motion is *dextrorsum*, that term being taken in the sense defined in art. 27. For under these circumstances the magnetic current and the circular part of an *ascending* current are opposed to each other on the *east* side of the wire and concur on the *west* side, and consequently, by the hydrodynamics of steady motion, the result is an excess of pressure *westward*; and similarly, the magnetic current and the circular part of a *descending* galvanic current are opposed on the *west* side of the wire and concur on the *east* side, and the consequent action is *eastward*. These inferences agree with the experimental facts; and accordingly this argument is decisive as to the direction of the spiral motion on the above hypotheses respecting the directions of the currents.

33. Supposing, in accordance with this conclusion, that the spiral motion is always *dextrorsum*, we may proceed next to account for the results of Oersted's experiment on the same hypotheses. The horizontal component of the earth's current flowing southward, it will follow that the end into which the proper streams of a magnet flow in converging courses is that which, when the magnet is suspended horizontally, points *northward*; for it is under these conditions that the terrestrial current gives that end a northward direction, as may be thus shown. Conceive the north end to deviate from the magnetic meridian through a certain angle *westward*, and let the earth's current and the above-mentioned converging streams be both resolved perpendicularly to the axis of the needle. Then the resolved parts will be in opposite directions on the *west* side of the north end, and in the same direction on the *east* side, and thus the north end will be driven *eastward*. Similarly, since the needle's streams are divergent from the south end, this end will be driven *westward*. Just the opposite effects would take place if the deviation of the north end were eastward. Accordingly the needle is in a position of stable equilibrium when it points northward and the marked end is in the magnetic meridian. This theory accounts for the directive action of the earth's magnetism.

34. Now let a galvanic current be caused to pass *from south to north* along a conducting-wire placed *over* the needle in the magnetic meridian, as in one case of Oersted's experiment. (See the figure in art. 627 of 'Ganot,' which is the same as that in art. 698 of Atkinson's edition.) Then supposing the con-

verging streams of the magnet at the north end and the circular part of the galvanic current to be resolved in a horizontal plane passing through the axis of the needle and perpendicularly to that axis, the two portions will concur on the *east* side of the needle and be opposed to each other on the *west* side, so that the needle will be urged *eastward*. Similarly at the south end the circular part of the galvanic stream and the resolved parts of the divergent magnetic streams concur in direction on the west side and are in opposite directions on the east side, and that end will thus be urged *westward*. Hence on both accounts the north end deviates towards the *east*. But according to Ganot and Atkinson the deviation in this case of the experiment is towards the *west*. What, then, is the explanation of this disagreement? Simply that the experimental result, as given by Ganot, is inconsistent with the course of the current from the zinc to the copper, as indicated by the signs + and - and by arrows. The result for the same case of the experiment, as stated by M. de la Rive (*Traité de l'Electricité*, vol. i. p. 207), and by Mr. Airy (*Treatise on Magnetism*, top of p. 210), accords with the above deduction from the theory in giving an *eastward* deviation. It is, further, to be remarked that Ganot's statements (in art. 627) of the results of the experiment in the four cases, together with the indicated directions of the current, are in agreement with Ampère's well-known rule for determining the direction of the deviation of the north end of the needle, and would also agree with the hypothesis of spiral motion, if that motion might be assumed to be *sinistrorsum* when the direction of the current is from the zinc to the copper. But this supposition is inadmissible, because experiment taken in conjunction with the theory shows conclusively that either the current is from the zinc to the copper and the spiral motion *dextrorsum*, or from the copper to the zinc and the spiral motion *sinistrorsum*; but neither theory nor experiment appears at present to be capable of deciding which of these laws is the true one. In any case the inconsistent statements of experimental results I have referred to, which have caused me a great deal of perplexity, require to be rectified.

All the other cases of Oersted's experiment may be similarly explained by the hydrodynamical theory on the same hypotheses.

35. One of the most remarkable phenomena relating to magnetism is the effect which a mass of copper in the neighbourhood of a magnetic needle has upon the number and extent of its vibrations. The hydrodynamical theory, combined with a simple magneto-galvanic law established experimentally by Faraday, offers the following explanation of this fact and of others of the same class. Faraday found that when a plate of copper ( $1\frac{1}{2}$  inch wide,  $\frac{1}{5}$  of an inch thick, and 12 inches long) was



placed with its faces at right angles to the line of junction of the poles of a powerful horseshoe magnet, and the terminals of a galvanometer were put in contact with the long edges, a galvanic current was developed as soon as the plate was caused to move transversely to the magnetic current (Phil. Trans. 1832, p. 151). Now, according to hydrodynamics, the displacement of the æther by the finite spherical atoms of the copper in motion would give rise to a stream of æther in the *opposite* direction, due to the reaction of the unlimited fluid mass. Hence, conceiving the plate to be held with its faces horizontal and the long edges parallel to the meridian, on moving it *northward* or *southward* a *southward* or *northward* current would be produced, which would coexist with the original magnetic current. The galvanometer, in fact, indicates that under these circumstances a *galvanic* current is generated, which flows from one of the long edges of the plate and completes the circuit by entering at the opposite edge. It follows from this fact, taken in conjunction with the theory, that the motions in rectangular directions of the two above-mentioned currents are partially converted into dextrorsum circular motion parallel to the meridian, and that this effect is attributable to the molecular constitution or arrangement of the atoms of the copper, although theory is at present incapable of ascertaining the exact *modus operandi*.

36. It is also found by experiment that if the magnet be moved and the copper be stationary, a galvanic current is equally produced. This fact appears to admit of the following explanation. The magnetic current and its lines of motion necessarily partake of the motion of the magnet. Hence relatively to the stationary atoms of the copper the current is a composite one, the horizontal component of which flows in the direction of the motion of the magnet and with the same velocity. Now, by hydrodynamics, this horizontal stream generates, by reason of the contraction of channel by the atoms and the inertia of the fluid mass, a *secondary* stream flowing in the *same* direction as the stream itself. Thus, besides the vertical current, there is a horizontal current flowing in the direction of the magnet's actual motion, and, therefore, in the direction *contrary* to that of the virtual motion of the copper *relative* to the magnet. Consequently the conditions for generating galvanic currents are exactly the same in this case as when the copper was moved and the magnet was at rest.

37. On the principles thus established we may proceed to explain the whole of the class of phenomena which depend on the relative motion between a magnet and a mass of copper. But for this purpose it will be necessary to ascertain previously the *direction* of the flowing of the current in Faraday's elemen-

tary experiment above described. This point is left in ambiguity by the experiments, as not admitting probably of being decided except by the combination of experiment with a true theory. The present theory furnishes for deciding it the following considerations.

38. In the description of the fundamental experiment Faraday states (referring to fig. 16 in plate iii. p. 131, of the Phil. Trans. for 1832) that "when the galvanometer-needle was deflected, its north or marked end passed eastward, indicating that the wire A received negative and the wire B positive electricity." As the wire A belongs to the terminal applied to the *eastern* edge of the plate and the wire B to that applied to the *western* edge (the two wires being in fact a single wire constituting the circuit), I was led by the above statement to suppose that the current proceeded out of the plate on the *west* side and entered it on the *east* side (Principles of Physics, p. 636). But on the assumption that the spiral motion of the galvanic current has been correctly determined by the foregoing argument to be *dextrorsum*, the truth of that supposition can be tested as follows by the observed direction of the displacement of the galvanometer-needle. The wire proceeding from the western edge was made to pass *beneath* the needle in the direction from south to north. Hence the circular motion about the rheophore would conspire on the *west* side of the *north* end of the needle with the entering magnetic streams resolved perpendicularly to the axis of the needle, and be opposed to them on the *east* side. Thus there would be an excess of pressure on the east side, and the deflection would be *westward*. But Faraday says that the marked end "passed eastward." It must therefore be concluded, in order to reconcile the theory with experiment, that the direction of the current is the reverse of that assumed, proceeding out of the *east* side and entering at the *west* side. It is true that the fact and theory would agree if the spiral motion might be assumed to be *sinistrorsum*; but the previous argument is opposed to this supposition. There remains, therefore, only the inference that the current actually flows along the rheophore from the east to the west side of the plate. Moreover, as will presently be shown, the phenomena of the mutual action between a magnet and a mass of copper relatively in motion are explainable by the theory only in case the current has this direction, which will at least give evidence of the consistency of the theory with itself.

39. Let us take the case of a circular plate of copper placed under a magnetic needle and caused to rotate about an axis coincident with that of the magnet, and let the direction of rotation be from west through north to east, or that of the move-

ment of the hands of a watch. Then conceiving at first the copper to be at rest, let the magnetic streams which pass through it, as well those of the magnet as the terrestrial streams, be resolved into components parallel and perpendicular to the faces of the plate. It is clear that the vertical component of the terrestrial magnetism can have no rotational effect; and the same is the case with respect to the horizontal components of both kinds, inasmuch as these only give rise to *vertical* galvanic currents, the effects of which, as regards rotation, neutralize each other. We have, therefore, only to consider the vertical components of the magnet's streams. Now as these streams enter the needle at the north end and the needle is above the copper, the vertical components will flow *upwards* at that end. Again, the copper being now supposed to rotate in the direction above stated, since the motion of the parts under the north end is *eastward*, the secondary or induced stream will flow *westward*. Comparing, therefore, these circumstances with the case of the elementary experiment in which, when the magnetic current flowed *upwards* and the induced current *southward*, the resulting galvanic current flowed *eastward* (see art. 38), it will appear, by imagining the horizontal directions to be turned through  $90^\circ$ , that in the actual case the galvanic current flows *southward*. With respect to what takes place at the south end, we have to consider that the vertical components of the magnet's issuing streams pass through the copper *downwards*, and that the direction of the induced streams is changed from westward to *eastward*. Each of these changes (as was shown by Faraday's experiments) causes a reversion of the direction of the galvanic current, and consequently the direction is *southward* at the south end as well as at the north end.

After this determination we have only to inquire what effect a *southward* galvanic current has on a magnetic needle above it. The spiral motion being dextrorsum, the circular motion will conspire with the entering streams at the north end on the *east* side, and be opposed to them on the *west* side. Hence the north end will be driven *eastward*, that is, in the direction of the motion of the disk. So the circular motion will be opposed to the issuing streams at the south end on the *east* side and conspire with them on the *west* side, and the south end will consequently be urged *westward*—that is, again in the direction of the motion of the disk. These results agree with the known facts of the experiment; and this agreement, it is to be noticed, depends upon the foregoing inference respecting the direction of the induced galvanic current.

40. All the other instances of the mutual action between a magnet and copper relatively in motion may be referred to the



same principles as those adopted and exemplified in the foregoing case; and their phenomena may be similarly accounted for.

41. To complete this review of the Hydrodynamical Theory of Magnetism, it remains to discuss and correct the theoretical explanations I have proposed respecting certain phenomena of Terrestrial Magnetism. At the end of the article "On Atmospheric Tides" in the Number of the Philosophical Magazine for January 1872, I have intimated my abandonment of the hypothesis that the lunar-diurnal variation of terrestrial magnetism is attributable to gradations of density of the atmosphere produced by the moon's gravitational attraction. Having in that article succeeded in solving with sufficient generality the problem of the disturbance of the atmosphere by the moon, I found that neither the law nor the amount of the gradation of density due to the moon's attraction could account for the facts of the lunar-diurnal variation. Being thus compelled to seek for another explanation, I reconsidered the views advanced in my first essay towards a theory of magnetism published in the Numbers of the Philosophical Magazine for January and February 1861, and have come to the conclusion that the gyratory motions of the æther there attributed to the rotations of the bodies of the sun, moon, and planets about axes are strictly deduced from the physical principles on which the theory is founded, and must accordingly be regarded as necessary consequences of the hypotheses of the theory. In fact I do not think that I could say on this part of the subject any thing different from what is said in arts. 27-30 contained in the February Number, which, after giving this reference, I consider it unnecessary to reproduce here.

42. With respect to all magnetism which has a *cosmical* origin, the view that I now take is that it is due to gyrations of the æther produced by the impulses it receives from the motions of the constituent atoms of the bodies of the solar system. The gyrations may either be immediately generated by the rotations of the bodies about their axes, or indirectly result from disturbances of the æther caused by their motions of translation. According to hydrodynamics, the motion of translation of a mass constituted atomically (as stated in art. 1) will continually impress on the æther motions whose mean direction is at the first instant directly *contrary* to that of the motion of the mass. This impressed motion will be subsequently converted into circulating or gyratory motion, because, according to the argument in art. 10, there can be no permanent transfer of any portion of the ætherial fluid across any fixed unlimited plane. Such circulating motions will necessarily partake of the motions of translation of the bodies which generate them, so as to have always the same geometrical relations to these bodies, provided their motions be uniform.

43. Accordingly the moon, since it completes relatively to the æther at rest a revolution about its axis in a month, will generate gyrations, the effect of which might possibly extend to the earth, and be perceived as a very small variation of terrestrial magnetism. But as these gyrations are in the direction of the moon's revolution about her axis, it will be found on trial that the disturbances of the magnetic needle which they produce are not in accordance with the observed law of the lunar-diurnal variation of magnetism. When, however, the effect of the disturbance of the æther by the moon's orbital motion is considered, it will be seen that as the impressed motion is tangential to the orbit and *contrary* in direction to that of the moon's motion, the generated circulating motion that reaches the earth will be *from the right hand to the left* of a spectator on the earth looking towards the moon, and therefore in the direction *opposite* to that of gyrations resulting from the moon's rotation about her axis. Also their effect, it may be presumed, would be much greater than that of these gyrations. Now the circulating streams produced as above stated by the moon's orbital motion would disturb the magnetic needle in a manner conformable with the law of the lunar-diurnal variation ascertained by observation. For when the magnet is on the meridian and under the moon, these streams, flowing *eastward*, would oppose the needle's entering streams at its north end on the *east* side, and the issuing streams at its south end on the *west* side, so that by both actions the western declination would be *increased*. The variation of the declination in any other position of the needle relative to the moon might on these principles be readily investigated; and it is easy to see that according to this theory there would be two maxima and two minima of declination in the course of twenty-four lunar hours. These deductions agree with the results of observation given in p. (ccxliii) of the Greenwich Observations of 1867.

44. The solar-diurnal variations of magnetism follow a different law, there being but one principal maximum in the course of twenty-four hours. This fact is accounted for by the circumstance that the sun's heat produces, *cæteris paribus*, a gradual diminution of the density of the atmosphere in all directions converging to the position of greatest heat, it being assumed, conformably with the argument in article 4, that such gradation of density, in consequence of its extending over a large space, is capable of generating magnetic streams of sensible magnitude. So far as this cause operates, we might expect that there would be magnetic effects due to the rapid changes of temperature and of atmospheric density in the day-time of a more decided character than any due to the slower changes in the

night-time; and this anticipation is confirmed by observation. But besides the magnetic variations due to changes of the temperature and density of the atmosphere, we may suppose that effects are produced by solar gyrations analogous to those attributed above to lunar gyrations. This cause might modify the epoch of day-maximum (which, as observation shows, does not occur at the hottest time of the day), and might also account for what has been called *the nocturnal episode*. As to the reality of the solar gyrations, I consider that we have ocular evidence in the phenomenon of the zodiacal light; for, according to the argument maintained in arts. 13 and 14, the rays which render the zodiacal light visible might originate in a collision between the gyratory motion and vibrations of different orders propagated from the sun, without its being necessary to suppose that matter other than the æther exists at points from which the rays proceed. It has been established by observation that the zodiacal light extends beyond the radius of the earth's orbit (see an article in the *Phil. Mag.* for February 1863).

45. The foregoing considerations enable me now to state the cause alluded to in art. 6, to which I conceive that minute annual inequalities of Dip, Total Force, and Declination might be attributed. Let it be admitted that the solar gyrations produce a sensible magnetic effect at the earth's distance from the sun. Then on the supposition that the gyrations are in circles symmetrically disposed relatively to the plane of the ecliptic (which must approximately be the case), it will follow that the magnetic effect varies with the earth's varying distance from the sun, being less as the distance is greater. Inequalities thus produced would be the same for the northern as for the southern hemisphere, agreeing in that respect with the small annual inequalities which, as stated in art. 6, General Sir E. Sabine has deduced from observation.

46. On the hydrodynamical principles which have been applied in the foregoing explanations, I make the following suggestion relative to the cause of the *secular variations* of terrestrial magnetism. From what has been already argued (art. 42), the motions immediately impressed on the æther by the earth's constituent atoms in consequence of its rotatory and orbital motions, result in circulating motions which, if the earth moved uniformly in a rectilinear course, would be steady motions having always the same geometrical relations to the position of the earth's centre. But the earth moves with varying velocity in a curved path. Assuming, therefore, that the system of circulating motions always tends to be steady and to maintain a rectilinear course, it is conceivable that there may be continual minute shiftings of the axis of the system in directions transverse to the



orbital motion and towards the tangential direction, analogous in some degree to the shifting of the earth's axis by precession and nutation. Such displacements of the axis of the ætherial movements would cause it to take positions relative to the earth continually more *westerly*.

47. I take this opportunity for rectifying an opinion respecting *earth-currents* which I have expressed in a note in p. 653 of my work on the 'Principles of Physics.' Since the note was written I have learnt from the Greenwich Observations that magnets and the galvanometers employed as indicators of earth-currents are simultaneously affected only by *large* magnetic disturbances, and that generally no correspondences exist between small magnetic variations and the indications of the galvanometers. It does not appear, therefore, that earth-currents and terrestrial magnetism have any special relation to each other, the effect of the large magnetic disturbances on the earth-current being only an instance of the ordinary mutual action between a galvanic current and a magnetic current.

Having completed this review of the hydrodynamical theory of magnetism, I think I may say that it is now supported by arguments which should command the attention of physicists. Considering the number and variety of the explanations of phenomena it gives, which in fact might have been much extended, I do not see how the inference that the facts of magnetism are referable to hydrodynamical laws can be resisted.

Cambridge, April 19, 1872.

### LII. *On the Moon seen by the naked Eye.*

By SAMUEL SHARPE, Esq.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

I SHOULD like to be allowed to lay the following conjectures before your readers. The opinions may not be true, and may not be new; but if new they seem worth consideration.

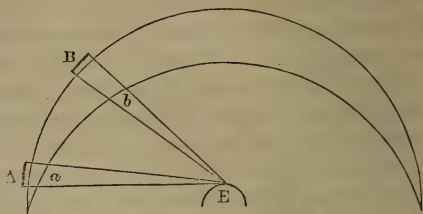
1st. As to the moon's atmosphere.

On looking at the new moon, when about two days old, with the naked eye on a clear evening, we see, first, the thin crescent, being that portion of the moon which enjoys sunshine; then the larger portion, nearly circular, dimly lightened up by earthshine. But the edge of the moon which is furthest from the bright crescent is also slightly illuminated. Now this portion of the moon seems to be in the twilight, illuminated by the sun's rays reflected from its own possible atmosphere. Though astronomers have not detected such an atmosphere by an effect on a star when

occulted, yet the above-mentioned appearance seems to prove its existence.

2nd. As to the enlarged appearance of the moon in the horizon.

Let the smaller semicircle E represent the earth. On the larger semicircle draw two small lines, A and B, of equal length, for the diameter of the moon at two different altitudes. From the extremities of these small lines draw conver-



ging lines to the eye of the spectator on the earth's surface. Now the astronomer, measuring the moon's diameter with his quadrant, finds that the moon in the horizon at A is smaller than the moon at B, as might be expected, because it is further off; but to the naked eye it always appears the larger of the two. This, I think, may be explained by remarking that on a fine night the vault of heaven never appears like half a globe, but is very much flattened over head. The effect of the atmosphere is to make the stars in the zenith seem nearer to us than the stars in the horizon. Let us draw such a flattened vault within our larger semicircle, and the small arcs *a* and *b*, intercepted by the lines drawn from A and B to the spectator's eye, will now represent our two moons on the apparent vault of heaven. Of these it will be seen that *a*, the moon near the horizon, is larger than *b*, the moon at a higher altitude, agreeably with the appearance in the heavens.

Yours obediently,

32 Highbury Place,  
13th May, 1872.

SAMUEL SHARPE.

LIII. *An Examination of the recent attack upon the Atomic Theory.* By R. W. ATKINSON, F.C.S., Assistant in the Chemical Laboratory, University College\*.

A SHORT time ago a paper was read by Dr. Wright before the Chemical Society, and subsequently appeared in the April Number of the Philosophical Magazine, entitled "On the Relations between the Atomic Hypothesis and the Condensed Symbolic Expressions of Chemical Facts and Changes known as

\* Communicated by the Author.

Dissected (Structural) Formulæ," in which he attempts to prove that "the conceptions involved in the atomic hypothesis are both unnecessary and insufficient." The following remarks are intended to show that whenever the author has the choice of two ideas, he invariably selects the one in accordance with the atomic theory, thus himself refuting the assertion that it is unnecessary. The answer to the argument of insufficiency is that the theory is a growing one, not one which has reached its limit; and the fact that nothing has been discovered which is inconsistent with it is strong evidence in favour of its truth.

After defining the terms "volume" and "vapour-density," the author deduces the volumetric ratio of the gaseous constituents of a compound from its composition, its vapour-density, and the densities of its constituents. He then asserts that "experiment shows that a volume of the vapour of almost all gasefiable compounds contains the gasefiable elements in the proportion of  $\frac{1}{2}$ ,  $\frac{2}{2}$ ,  $\frac{3}{2}$ ,  $\frac{4}{2}$ ,  $\frac{5}{2}$ , . . . . volumes." Taking at random one of the most recent determinations, including all the data required, let us see whether the volumetric ratio of the constituents of the oxychloride of tungsten conforms to this so-called experimental law. Analysis gave the percentage numbers :

Tungsten . . .	= 53.89
Chlorine . . .	= 41.11
Oxygen . . .	= 5.00
	<hr/> 100.00

Vapour-density from four experiments = 172.4. These numbers show that one volume of the oxychloride contains 0.539 volume of oxygen and 1.996 volume of chlorine; that is, the oxygen occurs in the ratio  $\frac{1}{1.85}$  or  $\frac{20}{37}$ , and the chlorine in the ratio  $\frac{1.996}{1}$ . These numbers approach the ratios respectively  $\frac{1}{2}$  and  $\frac{4}{2}$ ; but they differ sufficiently from them to compel us to adopt one of the alternatives, either they are the correct numbers or they are not. If they are the correct numbers, then the elements do not combine in definite volumetric ratios; if they are not, then in correcting them we assume the atomic theory. The theory of the atomic constitution of matter leads us to reject the experimental numbers and to adopt the simple ratios; but if we reject that theory, we are bound to retain the numbers obtained by experiment. The assertion of Dr. Wright, that vapours contain the elements in simple ratios, shows that he does not accept the first alternative, and consequently that he adopts (unconsciously no doubt) the atomic theory to account for the discrepancy between the theoretical and experimental



numbers. Hence it follows that in adopting two volumes as the unit of volume for compound vapours, which depends upon this assumption, he also makes use of atomic ideas; and upon this, again, he founds his definition of "combining numbers."

Dr. Wright refers to the law of multiple proportions as one of the facts of chemistry. Experiment, however, does not lead to numbers which are multiples of his "combining numbers." Thus, referring again to Roscoe's researches on the tungsten compounds, there are four chlorides described; and analysis shows that for 100 parts of the metal they contain the following amounts of chlorine:—

	W.		Cl.
1.	100	. . .	36.1
2.	„	. . .	73.8
3.	„	. . .	95.4
4.	„	. . .	114.7

or the ratio of the amounts of chlorine is

$$1 : 2.04 : 2.64 : 3.18,$$

or

$$2 : 4.08 : 5.28 : 6.36.$$

From this example it will be seen that the law of multiple proportions cannot be termed an experimental fact. In order to adopt the law, we must assume that these numbers are incorrect, and replace them by others which agree with the atomic theory. But in admitting the existence of this law, Dr. Wright does at the same time make use of the atomic explanation to account for the differences between the experimental and theoretical numbers.

We have seen the assumption involved in Dr. Wright's definition of "combining number;" but even after making it, the rule does not always lead to the number in use. Thus the vapour-density of ferric chloride would lead to the "combining number" for iron 112, and that of aluminic chloride to the number 55. These examples speak for themselves as regards the value of the rule given for the determination of the atomic weights of those elements which form volatile compounds.

For the determination of the "combining number" of non-volatile elements the rule given is even more fallacious. In the following Table are given the "combining numbers" of some elements which form volatile compounds, their specific heats, and the products of the two numbers.

	Comb. no.	Specific heat.	
	a.	b.	$a \times b.$
Carbon . .	12	0.147	1.76
Oxygen . .	16	0.155	2.48
Boron . .	11	0.25	2.75
Chlorine . .	35.5	0.093	3.3
Sulphur . .	32	0.163	5.22
Phosphorus .	31	0.174	5.39
Arsenic . .	75	0.0814	6.11
Antimony .	122	0.0523	6.38
Mercury . .	200	0.0319	6.38
Aluminium .	55	0.2143	11.74
Iron . . .	112	0.1138	12.74

Would it not seem that the numbers in the fourth column are too variable to serve as a foundation for the determination of "combining numbers?" It is true that Dr. Wright would except oxygen and chlorine from this list on account of difference of physical condition; but what about the other numbers, which vary from 1.76 to 12.74? Examples are not needed to show the fallacy of relying upon this fact alone for the determination of atomic weights; its use consists in deciding between equivalent weights and multiples of those weights.

Dr. Wright says that "the term combining number is extended to relative numbers *approximately* equal to  $\frac{6.6}{S}$ ." What is this but an admission of the worthlessness of his rule; for in the determination of such number approximations are not sufficient?

In assigning to oxygen the "combining number" 16, the author admits that in no compound do less than two equivalents of that element exist; that is, that that weight of oxygen is indivisible. In the same manner he allows the twelve parts by weight of carbon to be indivisible. But the admission of indivisibility is at the same time an admission of the existence of atoms, which he professes to dispense with, or to regard as unnecessary. The notion of atoms, however, cannot be unnecessary if Dr. Wright is obliged to make use of it, as it is evident he is.

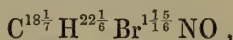
Dr. Wright defines a radical to be "one or more symbols and suffixes transferable from one formula to another;" and in another place he says, "the passage of a given quantity of electricity through an electrolyte causes the evolution of equivalent quantities of the radicals into which it decomposes the electrolyte, no matter what be the nature of the electrolyte." What does the author mean by equivalent quantities of an assemblage

of symbols and suffixes? He repeats his definition of a radical when he describes what he means by valency, which term, he says, "is applied to the radical to indicate the quotient obtained by dividing the combining number of the radical by its equivalent in the particular reaction in question," implying by this that valency is a function of an assemblage of symbols!

The author quotes Wurtz's definition of an atom and a molecule to show that there are two senses in which the term "atom" may be employed. He says that "in one sense the atom is a finite portion of matter of given weight, and hence possessed of dimensions in space, mass, and time," whatever the latter part of the sentence may mean. In the other sense he describes the atom to be "a pure number possessed of no dimensions in space, mass, and time." It would probably astonish Dr. Williamson to learn that he looked upon an atom as a pure number. Mathematicians have not yet discovered that it is a property of pure numbers to combine in definite proportions.

Dr. Wright says that the generalization of multiple proportions "is not identical with the hypothesis of the existence of material atoms, advanced to account for the facts summed up in the generalization; and to say that the law has no existence apart from the atomic hypothesis is to give a meaning to the term atom different from that attributed to it by Dalton." It has been shown above that the law of multiple proportions is not an experimental fact; it is only assumed when the discrepancy between the theoretical and observed numbers is accounted for by the use of the atomic theory.

Thus it follows that the method of finding the formula of a body from its percentage composition, being a deduction from Dalton's law, is also based upon the notion of the existence of atoms. Dr. Wright's analysis of the hydrobromate of bromocodeine would lead him to the formula



containing fractions of atomic weights, instead of the formula he adopts,



but instead of accepting the numbers obtained by experiment, he rejects them, and takes the nearest numbers which yield a formula containing only integral multiples of atomic weights. This shows that when an opportunity is offered to him of choosing between the atomic theory and its antithesis, whatever that may be, he accepts the atomic explanation.

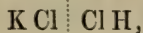
Dr. Wright does not touch the subject of direct and indirect combination, although this affords one of the strongest qualitative proofs of the existence of atoms. No compound is known of which



the molecule consists of an atom of hydrogen combined only with an atom of potassium; but in the molecule of caustic potash they are so firmly held together that they remain in combination even at a red heat. Neither the potassium nor the hydrogen can go off with its share of the oxygen; the latter is indivisible, binding together the two other atoms.



If, however, the atom of oxygen be replaced by an equivalent quantity of chlorine (that is, two atoms), then the atoms of potassium and hydrogen,



are separated, each going off in combination with an atom of chlorine.

Neither does he attempt to explain the cause of isomerism, which is so well done by the notion of the existence of atoms associated in different relative positions. Perhaps Dr. Wright will yet explain how he accounts for the differences between isomeric bodies.

It has now been shown, I submit, that, notwithstanding the author's assertion that the "conceptions involved in the atomic hypothesis are both unnecessary and insufficient," he yet makes consistent use of it in all his fundamental positions. This evidence in favour of the theory is doubtless unconscious; but it is not the less weighty on that account.

LIV. *On the Relations between the particular Integrals in Cayley's solution of Riccati's Equation.* By J. W. L. GLAISHER, B.A., F.R.A.S., Fellow of Trinity College, Cambridge\*.

IN the Philosophical Magazine for November 1868 (S. 4. vol. xxxvi. p. 348) Professor Cayley has given the solution of Riccati's equation in series which consist of a finite number of terms in the integrable cases.

Writing the transformed Riccati's equation in the form

$$\frac{d^2u}{dx^2} - x^{2q-2}u = 0, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

it is shown that the four following series are particular integrals:

\* Communicated by the Author.

$$u=P=\left\{1+\frac{q-1}{q(q-1)}x^q+\frac{(q-1)(3q-1)}{q(q-1)2q(2q-1)}x^{2q}+\dots\right\}e^{-\frac{x^q}{q}},$$

$$u=Q=\left\{1-\frac{q-1}{q(q-1)}x^q+\frac{(q-1)(3q-1)}{q(q-1)2q(2q-1)}x^{2q}-\dots\right\}e^{\frac{x^q}{q}},$$

$$u=R=x\left\{1+\frac{q+1}{q(q+1)}x^q+\frac{(q+1)(3q+1)}{q(q+1)2q(2q+1)}x^{2q}+\dots\right\}e^{-\frac{x^q}{q}},$$

$$u=S=x\left\{1-\frac{q+1}{q(q+1)}x^q+\frac{(q+1)(3q+1)}{q(q+1)2q(2q+1)}x^{2q}-\dots\right\}e^{\frac{x^q}{q}}.$$

If  $q(2i+1)=1$ ,  $P$  and  $Q$  are finite series, and the complete integral is  $u=CP+C'Q$ , a finite expression. Similarly, if  $q(2i+1)=-1$ ,  $u=CR+C'S$ , a finite expression. This solution therefore not only points out the integrable cases, but also exhibits the integral itself, and on this account is preferable to any other that has been given.

Now suppose  $q$  is unrestricted, we have four particular integrals,  $P, Q, R, S$ ; and since, if  $u=A$  and  $u=B$  are two independent particular integrals, the complete integral is  $u=CA+C'B$ , it follows that only two are independent, and that the other two are linear functions of them. It is the general determination (viz. (1) when  $q$  is not  $=\pm(2i+1)^{-1}$ , (2) when  $q=(2i+1)^{-1}$ , and (3) when  $q=-(2i+1)^{-1}$ ) of the independence of, and relations between  $P, Q, R, S$  which it is the object of this note to ascertain.

It will be convenient to write  $n$  for  $\frac{1}{q}$  and  $\beta$  for  $\frac{x^q}{q}$ ; the series thus become

$$P=\left\{1+\frac{n-1}{n-1}\beta+\frac{(n-1)(n-3)}{(n-1)(n-2)}\frac{\beta^2}{2}+\dots\right\}e^{-\beta},$$

$$Q=\left\{1-\frac{n-1}{n-1}\beta+\frac{(n-1)(n-3)}{(n-1)(n-2)}\frac{\beta^2}{2}-\dots\right\}e^{\beta},$$

$$R=\left\{1+\frac{n+1}{n+1}\beta+\frac{(n+1)(n+3)}{(n+1)(n+2)}\frac{\beta^2}{2}+\dots\right\}e^{-\beta},$$

$$S=\left\{1-\frac{n+1}{n+1}\beta+\frac{(n+1)(n+3)}{(n+1)(n+2)}\frac{\beta^2}{2}-\dots\right\}e^{\beta}.$$

If  $n$  be not of the form  $\pm(2i+1)$ , it will now be shown that  $P=Q$  and  $R=S$ :

The coefficient of  $\beta^r$  in  $P$  is

$$\begin{aligned}
 & (-)^r \left\{ \frac{1}{r} - \frac{n-1}{n-1} \frac{1}{r-1} + \dots (-)^r \frac{(n-1)(n-3)\dots(n-2r+1)}{(n-1)(n-2)\dots(n-r)r} \right\} \\
 &= \frac{(-1)^r}{(n-1)\dots(n-r)} \left[ \frac{(n-1)(n-2)\dots(n-r)}{r} \right. \\
 &\quad \left. - \frac{(n-2)(n-3)\dots(n-r)}{r-1} \cdot \frac{n-1}{2} \cdot 2 + \right. \\
 &\quad \left. \dots (-)^r \cdot \frac{\frac{n-1}{2} \left( \frac{n-1}{2} - 1 \right) \dots \left( \frac{n-1}{2} - r + 1 \right)}{r} \cdot 2^r \right];
 \end{aligned}$$

and the quantity in square brackets = coefficient of  $t^r$  in

$$\begin{aligned}
 & (1+t)^{n-1} - \frac{n-1}{2} \cdot 2t(1+t)^{n-2} + \dots \\
 & \frac{\left( \frac{n-1}{2} \right) \dots \left( \frac{n-1}{2} - r + 1 \right)}{r} (2t)^r (1+t)^{n-r-1} \\
 &= (1+t)^{n-1} \left( 1 - \frac{2t}{1+t} \right)^{\frac{n-1}{2}} = (1-t^2)^{\frac{n-1}{2}}.
 \end{aligned}$$

If therefore  $r$  be odd, the coefficient of  $\beta^r$  in  $P$  is zero; and if  $r$  be even, the coefficient is

$$\begin{aligned}
 & (-)^{\frac{1}{2}r} \frac{1}{(n-1)(n-2)\dots(n-r)} \cdot \frac{(n-1)(n-3)\dots(n-r+1)}{\left( \frac{1}{2}r \right) 2^{\frac{1}{2}r}} \\
 &= (-)^{\frac{1}{2}r} \frac{1}{(n-2)(n-4)\dots(n-r)} \cdot \frac{1}{\left( \frac{1}{2}r \right) 2^{\frac{1}{2}r}}.
 \end{aligned}$$

We therefore have

$$\begin{aligned}
 P &= 1 - \frac{\beta^2}{n-2} + \frac{1}{(n-2)(n-4)} \frac{\beta^4}{2^2 \cdot 2} \\
 &\quad - \frac{1}{(n-2)(n-4)(n-6)} \frac{\beta^6}{2^3 \cdot 3} + \dots, \quad (2)
 \end{aligned}$$

which evidently satisfies (1). Since then  $P$  involves only even powers of  $\beta$ , and  $Q$  is derived from  $P$  merely by changing the sign of  $\beta$ , it follows that  $P=Q$ .

In a similar manner it can be shown that

$$R = S = \beta^n \left\{ 1 + \frac{\beta^2}{n+2} + \frac{1}{(n+2)(n+4)} \frac{\beta^4}{2^2 \cdot 2} + \dots \right\} \quad (3)$$

When, however,  $n=2i+1$ , and  $P$  and  $Q$  are finite series, we do not have  $P_1=Q_1$  ( $P_1$  and  $Q_1$  denoting the terminated series in



this case), as is evident from the solutions of particular equations written down by Professor Cayley in his paper.

The relation in this case can be inferred thus: the series which multiplies the factor in  $P$  consists of a finite portion including the first  $i+1$  terms; after this point the terms vanish, owing to the presence of the factor  $n-2i-1$  in the numerator, until the same factor makes its appearance in the denominator, when the two factors cancel one another and the series recommences; this latter portion is

$$\frac{(n-1)(n-3)\dots 2(-2)\dots (-n+1)}{(n-1)(n-2)\dots 1} \left\{ \frac{\beta^n}{n} + \frac{n+1}{n+1} \beta^{n+1} + \dots \right\},$$

which after reduction becomes

$$(-)^{\frac{n-1}{2}} \frac{n}{(1.3.5\dots n)^2} \beta^n \left\{ 1 + \frac{n+1}{n+1} \beta + \frac{(n+1)(n+3)}{(n+1)(n+2)} \frac{\beta^2}{2} + \dots \right\}. \quad (4)$$

Denoting then by  $a$  the constant factor of this series, (4) =  $aR$ , as the series is the same as that in  $R$ , and we have  $P = P_1 + aR$ . Similarly  $Q = Q_1 - aS$ ; and since  $R = S$  (these series not terminating in this case of  $n = 2i + 1$ ), we have, since  $P = Q$ , when the whole series are taken into account,

$$P_1 + aR = Q_1 - aR,$$

and therefore

$$R = \frac{1}{2a} (Q_1 - P_1) = S. \quad \dots \quad (5)$$

The equality of  $P$  and  $Q$  depending merely on an algebraical multiplication (each being equal to (2)), we have good reason for asserting that they are equal so long as all the terms are taken into account. It is desirable, however, to prove the truth of (5) by the direct development of  $P_1$  and  $Q_1$  in powers of  $\beta$ ; and this can be effected by means of a theorem which I proved in the 'Quarterly Journal of Mathematics'\*, vol. xi. p. 267, and which in the notation of this paper may be stated,

$$P_1 = \frac{(-)^i \beta^n}{1.3.5\dots(n-2)} \left( \frac{1}{\beta} \frac{d}{d\beta} \right)^i \frac{e^{-\beta}}{\beta}.$$

To develop the continued differential, let  $\beta^2 = \alpha$ , so that

$$\left( \frac{1}{\beta} \frac{d}{d\beta} \right)^i \frac{e^{-\beta}}{\beta} = 2^i \left( \frac{d}{d\alpha} \right)^i \frac{e^{-\alpha}}{\sqrt{\alpha}},$$

\* "On Riccati's Equation." The value in the text for  $P$  is derived from the equation numbered (12) on p. 271 by making the same substitutions as those used in this paper, viz.  $\beta = \frac{x^q}{q}$  and  $q = n-1$ , so that  $x = \beta^{n-1}$  and  $x^{2q-1} dx = n^{-1} \beta d\beta$ .

and write  $\beta^2$  for  $\alpha$  finally after the differentiation; then

$$\left(\frac{d}{d\alpha}\right)^i \frac{e^{-\alpha}}{\sqrt{\alpha}} = \left(\frac{d}{d\alpha}\right)^i \left\{ \alpha^{-\frac{1}{2}} + 1 + \dots - \frac{\alpha^i}{(2i+1)} + \frac{\alpha^{i+\frac{1}{2}}}{(2i+3)} - \dots \right\}$$

$$= A_0 \alpha^{-\frac{1}{2}-i} + A_2 \alpha^{\frac{1}{2}-i} \dots + A_{2i} \alpha^{-\frac{1}{2}} - B_0 + A_{2i+2} \alpha^{\frac{1}{2}} - B_2 \alpha + \dots,$$

in which it is to be remarked that the terms in  $\alpha, \alpha^2, \dots \alpha^{i-1}$  give no terms on differentiation. There are, as it were, two series whose coefficients are distinguished by the letters A and B, the latter commencing from the term  $\alpha$ , and including  $\alpha^{i+1}, \alpha^{i+2}$ , &c. We thus find

$$P_1 = A'_0 + A'_2 \beta^2 + A'_4 \beta^4 + \dots - \beta^n (B'_0 + B'_2 \beta^2 + B'_4 \beta^4 + \dots);$$

and it will be shown that the first series = P, and the second = aR. To prove this, if  $r < i$ ,

$$A_{2r} = (-)^{i-r} 2^{-i} \frac{1.3 \dots \{n-(2r+2)\}}{2^r \lfloor r \rfloor},$$

so that

$$A'_{2r} = (-)^r \frac{1}{(n-2)(n-4) \dots (n-2r)} \frac{1}{2^r \lfloor r \rfloor};$$

if  $r$  be greater than  $i$ , then

$$A_{2r} = \frac{2^{-i}}{1.3 \dots (2r-n) 2^r \lfloor r \rfloor},$$

and

$$A'_{2r} = (-)^i \frac{1}{\{1.3 \dots (n-2)\} \{1.3 \dots (2r-n)\} 2^r \lfloor r \rfloor};$$

so that the first series = P, as is evident from comparison with (2). The second series

$$= \frac{2^i (-)^i \beta^n}{1.3.5 \dots (n-2)} \cdot \frac{\lfloor i \rfloor}{(2i+1)} \left\{ 1 + \frac{i+1}{(n+1)(n+2)} \beta^2 \right.$$

$$\left. + \frac{(i+2)(i+1)}{(n+1) \dots (n+4)} \frac{\beta^4}{2} + \dots \right\}$$

$$= \frac{(-)^i n \beta^n}{(1.3.5 \dots n)^2} \left\{ 1 + \frac{1}{n+2} \frac{\beta^2}{2} + \frac{1}{(n+2)(n+4)} \frac{\beta^4}{2^2 \lfloor 2 \rfloor} + \dots \right\}$$

$$= aR; \text{ therefore } P_1 = P - aR;$$

and by merely changing the sign of  $\beta$  we have  $Q_1 = Q + aR$ , whence (5) follows at once. If  $n = -(2i+1)$ , it will be found that

$$R = R_1 + aP, \quad S = S_1 - aQ;$$

so that

$$P = \frac{1}{2a} (S_1 - R_1) = Q,$$

$a$  being, as before, only suitably modified on account of  $n$  being negative.

The connexion between the integrals, if we regard the series as terminating when the numerators of the factors become zero, is therefore:—(1) if  $q$  is not  $= \pm (2i+1)^{-1}$ , then  $P=Q$ ,  $R=S$ , and  $P$  and  $R$  are independent; (2) if  $q=(2i+1)^{-1}$ ,  $P$  and  $Q$  are independent, and  $R=S=\frac{1}{2a}(Q-P)$ ; (3) if  $q=-(2i+1)^{-1}$ ,  $R$  and  $S$  are independent, and  $P=Q=\frac{1}{2a}(S-R)$ . But the proper way is to regard the series as including all their terms; and then the result merely is that we have always two independent particular integrals  $P$  and  $R$ , such that  $P \pm aR$  and  $R \pm aP$  are finite algebraical expressions in the respective cases of  $q=(2i+1)^{-1}$  and  $q=-(2i+1)^{-1}$ .

The integral made use of in the 'Quarterly Journal' to obtain the above expression for  $P_1$  was due to Poisson, and, altered so as to agree with the notation of (1), takes the form

$$u = \int_0^\infty e^{-z^{2q} - \frac{x^{2q}}{4q^2 z^{2q}}} dz,$$

which by a pair of simple transformations (which are given in the paper referred to) may be written in either of the forms

$$\int_0^\infty v^{n-1} e^{-v^2 - \frac{1}{4}\beta^2 v^{-2}} dv, \text{ or } \beta^n \int_0^\infty v^{n-1} e^{-\frac{1}{4}\beta^2 v^2 - v^{-2}} dv.$$

By expanding the exponential factor in the former of these and integrating each term by means of the integral

$$\int_0^\infty v^m e^{-v^2} dv = \frac{1}{2} \Gamma\left(\frac{m+1}{2}\right), \quad \dots \dots \dots (6)$$

we obtain the series (2). A similar treatment of the second integral and reduction by means of

$$\int_0^\infty v^m e^{-v^{-2}} dv = \frac{1}{2} \Gamma\left(\frac{-m-1}{2}\right) \quad \dots \dots \dots (7)$$

leads to the other series (3). Of course this method is not legitimate, as in both cases we must at length, whatever  $q$  may be, come to some point where the factor that multiplies the exponential (in the form  $e^{-v^2}$ ) under the integral sign is raised to a power  $< -1$ , when the term becomes really infinite and the method fails. But it is remarkable that if we ignore this failure and treat (6) and (7) as if they were universally true, we are led to both the series (2) and (3), and thence from a particular to a general integral of (1).

Trinity College, Cambridge,  
May 12, 1872.



LV. *On the Mode in which Stringed Instruments give rise to Sonorous Undulations in the surrounding Atmosphere.* By ROBERT MOON, M.A., Honorary Fellow of Queen's College, Cambridge\*.

THAT the tones of a stringed instrument are due to the action upon the air of the sounding-board, and in no perceptible degree proceed from the direct action upon the air of the strings themselves, may be taken to be one of the earliest discovered facts in acoustics, since it is clear that no such instrument of any of the kinds with which we are familiar could have been constructed in ignorance of it. But the mode in which the sounding-board acts on the surrounding atmosphere so as to give rise to sonorous undulations is a subject which presents extraordinary difficulties, which, so far as I am aware, have not been adverted to.

Consider the case of the grand pianoforte with the lid wholly removed.

The sounding-board consists of a single plate, which is shrunk into a frame in such a manner as to present a surface which is *convex upwards*.

The lower surface, which is exposed to the air, is crossed at intervals by narrow bands, which increase its rigidity and consequently its sonorous power, but which need not further attract our attention.

The strings, through the medium of the bridge which is firmly fixed to the sounding-board, exert a downward pressure on the latter, and thus bring it into a state of constraint †.

When a note is sounded, the hammer striking the wire upwards, the downward pressure on the bridge is relieved, and the sounding-board bounds upwards. Thus a wave of condensation will be propagated vertically upwards, and a wave of rarefaction vertically downwards.

The mode in which the vibrations of the board may be propagated vertically above or vertically below the instrument is thus conceivable enough; but what of the space outside of a vertical cylinder having the sounding-board for one of its sections? and how are these two diverse disturbances (the one a condensation, the other a rarefaction) to combine themselves into a spherical wave wholly of condensation or wholly of rarefaction, or compounded of condensation and rarefaction in immediate sequence, such as, to common apprehension, appears the alone sufficient agent for the diffusion of sound?

\* Communicated by the Author.

† For the above details I am indebted to the kindness of Mr. Bruzaud, of the firm of Messrs. Erard.

We can conceive the condensed wave extending itself horizontally beyond the limits of the cylinder of which we have spoken, and then descending outside the cylinder *below* the level of the sounding-board; but we shall have contemporaneously a corresponding horizontal extension of the rarefied wave, which must have the same faculty of extending itself *upwards* that the condensed wave has of extending downwards.

What will ensue upon the meeting of these two opposite disturbances?

One consequence of their meeting clearly will be that the density in the plane of the sounding-board will be that of equilibrium; so that in every direction in this plane we shall have an entire absence of that alteration of density which, according to the received theory of sound, is the sole agent in the creation and transmission of aërial vibrations\*. Hence, on the principles of that theory, the tones of the instrument might be expected to be inaudible at all points in the plane of the sounding-board—a phenomenon, it may be remarked, which has not hitherto been observed to occur.

But the subject admits of being considered in a different manner.

The amplitude of vibration of the sounding-board will be very minute, at the same time that the agitation of the air above it occasioned by a single bound upwards of the board will extend over a very considerable space. It is evident, therefore, that the condensation of the air vertically above the sounding-board will be extremely small; and it is quite conceivable that the principal influence of the air within the cylinder in moving the air without the cylinder may not be of the nature already described, but may be due to *lateral adhesion*.

Assuming this to be the case, it is clear that the particles of air around the instrument, whether within or without the cylinder above spoken of, and whether above or below the plane of the sounding-board, will have this feature in common, viz. a velocity vertically upwards. Hence it is easy to see how the air in every direction, upwards, downwards, and laterally, at any given instant may have a spherical arrangement of this kind, viz. that the particles in each spherical shell, described about the centre of vibration of the sounding-board as a centre, have the same absolute velocity in a direction vertically upwards.

\* It is curious to note that although in the plane of the sounding-board the condensation and rarefaction will mutually destroy each other, this will not be the case with the velocities of the two waves; so that the particles in that plane will have a vertical motion. I apprehend, however, that few will attempt to account for the transmission of sound along this plane on the principles of transversal vibration!

It may be thought that the interval which separates a state of disturbance of this kind from a spherical wave consisting of a shell of condensation immediately succeeded by a shell of rarefaction is little short of infinite.

If, however, it can be shown that in the case of motion parallel to the axis of a cylindrical tube filled with air, the pressure in part depends on the density and in part on the velocity; and if it can be shown further that under the same circumstances the portion of the pressure due to the velocity may be very considerable, while that due to the condensation is very small; I think it will be admitted that we shall have made a great advance, I do not say towards the solution of the problem, but in indicating the direction in which the solution must be sought.

Now in the case of motion last spoken of, putting  $p$ ,  $v$ ,  $\rho$  for the pressure, velocity, and density at the time  $t$  at a point the coordinate of whose point of rest is  $x$ , we shall have

$$\begin{aligned} p &= \text{funct. } (x, t), \\ v &= \text{funct. } (x, t), \\ \rho &= \text{funct. } (x, t); \end{aligned}$$

and, eliminating  $x$  and  $t$  between these three equations, we shall get

$$p = \text{funct. } (\rho, v),$$

thus proving that the pressure is dependent both on the velocity and density, and not, as is erroneously stated in the ordinary theory upon the subject, upon the density alone.

Moreover, putting  $y$  for the ordinate of the particle at the time  $t$ , and  $D$  for the mean density, the equation of motion of the particle will be

$$0 = \frac{d^2y}{dt^2} + \frac{1}{D} \frac{dp}{dx},$$

a conclusion which, I apprehend, no one will dispute.

I have elsewhere shown\* that this equation is satisfied by the three following relations, viz.

$$\begin{aligned} p &= -\frac{\alpha^2}{\rho} + \phi\left(v + \frac{\alpha}{\rho}\right), \\ v + \frac{\alpha}{\rho} &= \psi_1\left\{x - \frac{\phi'(u) - \alpha}{\rho} \cdot t\right\}, \\ \frac{1}{\rho} + \int \frac{du}{\phi'(u) - 2\alpha} &= \psi_2\left\{x - \frac{\alpha}{D} \cdot t\right\}, \end{aligned}$$

\* See Philosophical Magazine, vol. xxxvi. p. 27.



where  $\alpha$  is constant,  $\phi$ ,  $\psi_1$ ,  $\psi_2$  are arbitrary functions, and

$$u = v + \frac{\alpha}{\rho};$$

and since the expression for  $p$  may be put under the form

$$\begin{aligned} p &= \alpha v - \alpha \left( v + \frac{\alpha}{\rho} \right) + \phi \left( v + \frac{\alpha}{\rho} \right) \\ &= \alpha v + \phi_1 \left( v + \frac{\alpha}{\rho} \right), \end{aligned}$$

it is clear that, if these results are to be received, in the case of aërial motion in one direction at least, the portion of the pressure due to the velocity may be very considerable, while that due to the density is inappreciable.

With regard to the results themselves, I may observe that they flow as certainly from the equations of motion as the propositions in Euclid result from the definitions and axioms. Hence any objection to the results must apply equally to the equation from which they are derived. If the one be untrue, or true only in a qualified sense, the other must be equally untrue, or its truth must be equally qualified. The results can only be rejected on the ground of there being in the background some controlling principle which is not taken into account in forming the equation of motion. Until the existence of such a principle has been demonstrated, which, so far as I am aware, has never been so much as suspected, we are bound to admit every fact derivable from the solution.

In the foregoing observations I have endeavoured to indicate, in a very general way, the mode in which, as I conceive, the solution of the problem before us is to be sought. Before that object can be accomplished many circumstances will need to be taken into account which I have not touched upon\*; indeed a solution of the equations of aërial motion in two directions, such as that above given for the equation applicable to motion in one direction, would seem to be an indispensable preliminary.

Such a solution I am in a position to afford, but I must seek another opportunity for its exhibition, and for the discussion of the results flowing from it.

I desire to express, in conclusion, my conviction that the complete resolution of this problem will be found to involve that of others of very great, and in one instance of transcendent importance.

6 New Square, Lincoln's Inn,  
May 18, 1872.

\* For instance, the lateral action by which I have supposed the particles outside the cylinder spoken of to be moved *upwards* necessarily implies a corresponding *horizontal* action.

LVI. *On the Objections raised by Mr. Tait against my Treatment of the Mechanical Theory of Heat.* By R. CLAUSIUS\*.

IN reply to my "Contribution to the History of the Mechanical Theory of Heat" †, Mr. Tait ‡ has given the matter a turn which to me is agreeable. For he therein contests not the priority of my investigations, but their correctness; so that it is now no longer a question of historic and personal explanations, but of scientific elucidations, which, on account of the importance of the subject to which they relate, may perhaps not be without a more general interest. I will therefore take no notice of the somewhat irritated tone of the reply, and only take into consideration the matter it contains.

As already mentioned in my previous article, the axiom employed by me for the demonstration of Carnot's theorem modified, viz. *that heat cannot pass by itself from a colder into a hotter body*, was immediately acknowledged as correct by Sir W. Thomson, and since then has likewise been made use of by many other authors for the same demonstration. Mr. Tait, on the contrary, now declares it to be fallacious.

Of the two phenomena adduced by him for the refutation of the proposition, I will first discuss that of which he says that it gives an "excellent instance of the fallacy of the so-called axiom"—namely, the phenomenon that a thermoelectric battery worked with ice and boiling water is capable of raising a fine wire to incandescence.

In one of my memoirs §, I have myself applied the mechanical theory of heat to thermoelectrical phenomena. I therein showed that a thermoelectric element (and so, of course, a thermoelectrical battery) may be compared to a steam-engine, the heated junction corresponding to the boiler, and the cold junction to the condenser. At the hot junction heat is withdrawn from a heat-reservoir, the temperature of which we will name  $t_1$ ; and at the cold junction heat is given up to another heat-reservoir, the temperature of which may be called  $t_0$ . But the quantity of heat given up is something less than the heat received; and hence the quantity of heat given up in the unit of time we will denote by  $Q$ , and the heat received by  $Q + q$ . The portion  $q$  of the latter quantity is expended in the work necessary for the production of the electric current; and the other portion,  $Q$ , passes from a body of the temperature  $t_1$  into a body of the temperature  $t_0$ .

When the work performed by a steam-engine is applied to the

\* Translated from the MS. communicated by the Author.

† Phil. Mag. February, 1872.

‡ Ibid. May 1872.

§ Pogg. Ann. vol. xc. p. 513.

overcoming of friction or other passive resistances, it is again transformed into heat, and under suitable conditions can generate a temperature far higher than that of the boiler. Just so in the electrical battery, the work which must have been accomplished in order to put the electricity in motion can be again transformed into heat in overcoming the resistances to conduction, and here also, under suitable conditions, can generate a much higher temperature than that of the heated junctions. For example, as Mr. Tait says, if the heated junctions have only the temperature of boiling water, a wire can be heated to incandescence.

Let the temperature which the wire acquires, and which can be maintained constant as long as we please, be denoted by  $t_2$ , then we can say that a part of that quantity of heat,  $q$ , which in the battery is expended for work appears again as heat in another body at the temperature  $t_2$ . As, then, the heat expended for work is derived from a heat-reservoir of the temperature  $t_1$ , we obtain as a result of the process the passage of a certain quantity of heat from a body at the temperature  $t_1$  into a body at the higher temperature  $t_2$ .

Now the question to be decided is, whether this passage of heat from a lower to a higher temperature has taken place *by itself*.

By this concise designation, *by itself*, I mean, as I have repeatedly explained, *without the simultaneous occurrence of another change serving for compensation*. So far as we have to do with cyclical processes, there are two sorts of changes which may serve for compensation, namely:—first, the passage of heat from a hotter into a colder body; and, secondly, the consumption of work, or, to express it more definitely, the transformation of work into heat.

If now from this point of view we contemplate our thermoelectric battery with the thin conducting wire which is brought to incandescence, we see that certainly part of the quantity of heat  $q$  passes over from the temperature  $t_1$  to the *higher* temperature  $t_2$ , but that simultaneously the other quantity  $Q$  passes from the temperature  $t_1$  to the *lower* temperature  $t_0$ . This latter passage of heat forms the compensation of the former; and hence we cannot say that the former has taken place *by itself*.

The case here discussed is so simple and clear that it might be chosen as a perfectly suitable example for the elucidation and confirmation of my axiom; and yet this is the case which Mr. Tait has selected as a demonstration of its fallacy.

Mr. Tait adduces as another case in contradiction to my axiom a thermoelectric circuit in which the hot junction is at a temperature higher than the neutral point. Consequently the circuit



in question is one in which the intensity of the current is not continually increased by greater heating of one junction, but from a certain temperature upward the current again diminishes in intensity, and with a still further rise of temperature may even change its direction.

This phenomenon I have likewise already discussed, in my above-mentioned memoir. I have endeavoured to explain it by the assumption that, in one of the two metals of which such a circuit consists (or even in both), the change of temperature gives rise to a change of molecular state, the effect of which is that the altered and unaltered portions of the metal have the same electrical relation to each other as two different metals. As soon as a change of this kind occurs, electromotive forces act not only at the places of contact of different metals, but also wherever differently constituted portions of the same metal are in contact. Accordingly heat will be generated or expended not merely at the junctions, but also in other parts, in the interior of the individual metals; and hence, in order to determine all the passages of heat that occur, we must consider not merely the temperatures of the junctions, but also the temperatures of those other parts.

Of course the thing becomes thereby more complicated. Besides, of the alterations mentioned, although their existence in individual cases has already been shown, we have yet too little special knowledge to be able to trace in detail all that take place in such a thermoelectric circuit. Meanwhile it will not be disputed that in the assumption I have made we have at least a possible explanation; and at any rate it will be admitted that a phenomenon in which circumstances as yet unknown cooperate is ill adapted to be used as a proof for or against a theorem advanced.

Finally Mr. Tait says further that by the introduction of the ideas of *internal work* and *disgregation* I have done harm to science.

What Mr. Tait has against the notion of *internal work* is to me incomprehensible. Since, in my first memoir on the mechanical theory of heat, I distinguished the work accomplished by heat, in the change of state of a body, into *external* and *internal* work, and then showed that these two quantities of work essentially differ in their behaviour, this distinction has been in like manner employed by all the authors who, to my knowledge, have written on the mechanical theory of heat.

With respect to the notion of *disgregation*, investigations by Boltzmann and myself have just been published by which it acquires a universal mechanical significance; and although the investigations relative to this subject are not yet concluded, I

believe they already make manifest that the introduction of this idea was dictated by the nature of the subject.

In conclusion, I will take the liberty to state again expressly the points to which Mr. Tait objects, in order to prevent any subsequent shifting of the position. Mr. Tait maintains:—

(1) That the theorem, that heat cannot by itself pass from a colder into a hotter body, is fallacious; and

(2) That the introduction of the notions of *internal work* and of *disgregation* was detrimental to science.

The decision on these two points, so far as they are at all still doubtful, I think I may confidently leave to the future; and thereby this controversy also will finally and fully decide itself.

Bonn, May 1872.

LVII. *On the Origin of the Earth's Magnetism, and the Magnetic Relations of the Heavenly Bodies.* By F. ZÖLLNER.

[Continued from p. 365.]

13.

IT is to be expected, according to the views above developed as to the physical causes of the earth's magnetism\*, that there exists a connexion between all the phenomena related to this magnetism and the volcanic appearances on the surface of the earth. In fact, if these processes are to be looked at according to the principles of every rational geology, as reactions of the glowing liquid nucleus of the earth on its solid crust, these reactions must manifest themselves as well by mechanical as by electrical and magnetical effects. The first of these effects are observed in the earthquakes and volcanic eruptions, the latter in the oscillations of the magnetic needle. The close connexion of these two kinds of appearances which is necessarily demanded by our theory is confirmed by numerous observations. I will

\* Whilst this paper passes through the press I receive the news of a very remarkable observation which was made a short time ago in France at the boring of a very deep artesian well. The Academy of Paris has received on this subject a communication, printed in the *Comptes Rendus*, vol. lxxiii. p. 910, under the title "Etude de l'eau artésienne de Rochefort Note de M. Roux (extrait)." The passage in question is the following:—"Une particularité intéressante, que nous avons observée pendant les travaux artésiens est l'aimantation énérgique de la sonde. Ses tiges désarticulées après le travail, constituaient autant d'aimants partiels, ayant chacune son pôle boréal et son pôle austral." Considering the weak magnetism of the minerals, this curious observation can, I think, only be explained by the existence of strong electrical currents in the earth, which would agree with the laws of the ramification of currents relative to their intensity.

Note by the Translator.—I have been told that an earthquake at Lisbon in the year (1858?) was coincident in point of time with a considerable disturbance of the magnets at the Kew Observatory.

only give in proof of this the facts which Lamont describes in his often-cited work (p. 277) in the following words:—

“Kreil has given many cases where magnetic disturbances coincided with earthquakes; hence he thinks a connexion between the two phenomena probable. I have observed myself an extremely curious case in this respect. On the 18th of April 1842, at 10 minutes past 9 o'clock in the morning, I saw by chance that the needle of the declination-instrument received a sudden jerk, so that the scale was pushed out of the field of view of the telescope. The oscillations continued for some time; at last the ordinary tranquillity was restored.

“After some days I received the news from Colla, in Parma, that he had observed violent oscillations of the needle; and comparison showed that the movement had begun at the same moment in Parma as in Munich. A short time afterwards the report of a French engineer was published on a violent earthquake which he had observed in Greece; and now it was found that the earthquake had taken place in the same minute in which the oscillations of the needle had been observed in Parma and Munich. This, together with the many cases collected by Kreil and Colla, leaves scarcely any doubt as to the presence of a closer connexion; but it is undecided whether one phenomenon is the consequence of the other, or whether they come both from the same source.”

The same connexion between earthquakes and magnetic disturbances was observed by Lamont at the earthquake which took place in Greece in December 1861. He communicates his observations to Poggendorff's *Annalen* (vol. cxv. p. 176) in the following words:—

“As the connexion of the magnetism of the earth with earthquakes still belongs to the insufficiently ascertained relations, it will not appear irrelevant if I communicate a fact bearing upon this question. On the 26th of December, 1861, at 8 o'clock A.M., when I took down the position of the magnetical instruments (six of which are put up in the magnetical observatory, viz. two for declination, two for intensity, and two for dip), I observed in all the instruments an uncommon restlessness, consisting in a quick and irregular decrease and increase in the declination, and at the same time a trembling in the vertical direction. The trembling of the needle only lasted for a short time; but the quick changes lasted until 8½ o'clock with gradually increasing violence. Some days later the news was received of an earthquake which, exactly coincident with the above observation, had caused great destruction in many parts of Greece.

“Hereby it is again established, not only that the shaking of the earth which is caused by an earthquake is propagated



to great distances, but also that the forces which cause the earthquake modify in a certain degree the magnetism of the earth. The modification consists doubtless in the production of an earth-current, which in the above case was so far confirmed, as the instruments at the observatory in this town indicating earth-currents showed at that time unusual activity."

## 14.

The phenomena which we have discussed embrace in general all those facts which, independent of the relations of the earth to other heavenly bodies, express merely a relation of terrestrial conditions and changes to terrestrial magnetism. In the following I have the intention to discuss as well those phenomena which prove in a definite manner a magnetical relation between the earth and other heavenly bodies.

As, according to our theory, the streaming process in the glowing liquid nucleus of the earth is only a repetition of the streaming process on the sun's surface, the same causes must lead to the same consequences.

*The sun therefore is, for these reasons, to be regarded as a magnetic body like the earth, whose poles of rotation in general do not coincide with the magnetic poles.*

But assuming the same relation between the direction of the liquid and electrical currents in the liquid solar surface, it follows that the poles of the sun have the opposite magnetic polarity to the corresponding poles of the earth; for as the solar surface does not yet possess a solid crust, and according to the law of rotation the currents on it are generated by friction against the polar undercurrents of the atmosphere, these north-easterly currents have the opposite direction to the south-westerly equatorial currents in the inner part of the earth. Considering, therefore, the rotation of the sun to be in the same direction as the rotation of the earth, the former must have a magnetic south pole where the latter has a magnetic north pole.

As regards the layers in which the generated electrical tensions are equalized by electrical currents, the inferior layers of the sun's atmosphere, which are dense and rich in vapour, take the place of the under parts of the earth's crust. It is not improbable that we observe this restoration of the electrical equilibrium in the form of protuberances, especially the protuberances which, as it were, cover with a bridge deeper-lying parts, and through the darkness of the covered space have given rise to the supposition of dark protuberances\*.

\* The observations of protuberances made by Respighi, Tacchini, C. A. Young, Norton, and others contain numerous indications of the similarity of the images of certain protuberances to the aurora borealis; and the inner

Generalizing the results we have arrived at, taking into account the general similarity in the history of development of all the larger heavenly bodies, we may express this generalization in the following terms:—

*All rotating heavenly bodies possess magnetical poles which do not coincide exactly with the poles of rotation. During the gradual cooling the polarity changes signs, so that a glowing liquid body possesses the opposite polarity to that of one which is covered by a solid crust.*

The magnetic polarity disappears:—first, when the change of signs takes place, *i. e.* in that phase where, as in the stars of variable brightness, the slag-like masses have already taken the character of extended continents; and secondly, when the nucleus has become entirely solid.

After these considerations the existence of a magnetical relation between the planets and the sun is, from the stand-point of my theory, to be regarded, at least qualitatively, as a physical necessity. Only observation can give us information as to the quantitative conditions. In the following it shall be investigated how far the above theoretical considerations can account for the general character of those relations, so far as it can be legitimately deduced from the observations.

15.

The sun's axis of rotation is, according to the observations and calculations of Spörer, inclined at an angle of  $6^{\circ} 57'$  to the plane of the ecliptic. The longitude of the ascending node of the sun's equator is (in 1866.5)  $74^{\circ} 36'$ . From this the longitude of the sun's north pole is found to be  $164^{\circ} 36'$ , and that of the south pole  $344^{\circ} 36'$ . Supposing a plane to be laid through the sun's axis perpendicular to the plane of the ecliptic, the earth in its path round the sun will cut this plane on two days, on September 6 and March 7. On the first day the sun turns its north pole, on the latter day its south pole, towards the earth.

It follows therefore that all the effects produced on the earth's surface by a magnetic induction of the sun must have a maximum at these two times of the year.

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part of the corona seen at total eclipses shows also this similarity. Lamont has drawn our attention to this with regard to the extremely great variability of the protuberances, which is also observed in the aurora borealis.

But it must always be borne in mind that this analogy regards only a certain class of protuberances; in others the eruptive character of volcanic phenomena is almost established beyond a doubt. On the classification of protuberances into those of cloudy and those of eruptive formation compare my paper "On the Temperature and Physical Constitution of the Sun," Proceedings of the Royal Saxon Society of Sciences, June 2, 1870. *Phil. Mag. S. 4. vol. xl. p. 313.*

*Phil. Mag. S. 4. Vol. 43. No. 288. June 1872.* 2 G

Let us recall to our minds the kind of effects which could be produced in the earth by such an induction. They are two-fold, viz. :—

1. Mechanical effects, by changes in the velocity of the glowing streams in the earth.

2. Magnetical or electrical effects, necessarily connected by our theory with these mechanical changes.

As to the first of these two influences, it is clear that, if the origin of terrestrial magnetism is really to be sought in the glowing streams and the electrical currents necessarily produced by them, any increase or decrease in the magnetism by induction must produce an increase or decrease in the velocity of these glowing streams.

It has already been shown above how the magnitude of the disturbing influences must vary with the magnitude of velocity of these streams, and how, therefore, at the time of the greatest magnetic induction a maximum of earthquakes, of magnetic disturbances, and of auroræ boreales must take place, which latter are produced by electrical induction in the rarefied regions of our atmosphere.

Theoretically, changes in the temperature of the earth must accompany these phenomena which manifest themselves on the earth's surface. For, as we have already remarked (p. 349), the increase or decrease in the *vis viva* of the streams can, according to the principles of the conservation of energy, only take place if an equivalent amount of *vis viva* disappears in another part of the system. Any large and not wholly solidified heavenly body presents us, besides its transitory and rotatory motion, its store of energy in two forms :—

1. In the form of heat, which is continually diminishing by radiation.

2. In the form of mechanical motion in the currents of its liquid and gaseous constituents.

If one of these two forms of energy is increased or diminished without exterior communication of *vis viva*, it must be at the expense of the other.

If, therefore, by magnetical induction the velocity of motion is increased, it must be accompanied by a decrease of temperature, and *vice versâ*.

Suppose therefore a physical cause to exist by which the magnetic condition of the sun is periodically changed. According to what has been said, a variation in the earth's temperature must be produced by the simultaneous variations in the magnetic induction of the earth. This change of temperature will be the more perceptible the deeper the thermometer is let down into the earth, and the nearer therefore it is to the glowing liquid interior.



Such a physical cause, which, according to our theory, must necessarily change periodically the magnetic condition of the sun, is really present; I mean the periodically changing quantity of sun-spots.

In my discussion of the law of rotation of the sun and that of the large planets\*, I have shown theoretically that the presence of sun-spots on the sun's surface must generate essential but regular modifications of the law of rotation. These modifications have been proved to exist by the observations of Spörer; so that I have succeeded in giving for the modified law a theoretical formula which represents more accurately the observed states than the empirical formula of Spörer.

The essential character of the law of rotation changed at the time of sun-spot maximum, and the causes which produce the alterations, were explained by me (*l. c.* p. 82) in the following words:—

“At the time of the sun-spot maximum the polar currents are considerably retarded in consequence of the increased friction; these currents will therefore arrive at the equator with a much smaller velocity than they do at the time of a sun-spot minimum. Hence at the times of sun-spot maxima the accelerating effect of the deeper strata of the sun's surface must be much larger than at the time of a minimum; and for this reason the observed velocity of rotation at the equator must be greatest at the time of a maximum—just as is shown by Spörer's observations.”

P. 85. “As we have seen, all the observations of Spörer can be much better represented by the above theoretical formula than by his own empirical formula. Excluding the observations for the immediate neighbourhood of the equator, the sum of the least squares of errors for Spörer's formula is 85·2, for mine 44·8, or little more than half of the value for Spörer's formula.

“The derived theory of the law of rotation, even near the time of a maximum of sun-spots, has therefore been confirmed by Spörer's observations; and the above formula will therefore have to be regarded in future as a quite general expression for the modifications of that law.”

*It is therefore a necessary consequence of my theory that at the times of sun-spot maxima the relative velocity and the friction of the currents is increased; and hence the intensity of the electrical currents produced, as well as the magnetism of the sun, is greatest at those times.*

At the times of sun-spot maxima the mechanical, electrical, and magnetical energy present in the sun is increased at the expense of a certain quantity of heat.

\* *Proc. of Roy. Saxon Soc. Sci.* Feb. 11, 1871.

In order to allow the reader to use his own judgment as to how far the phenomena flowing theoretically from a periodic change in the sun's magnetism agree with the facts observed on the earth's surface, I shall take the liberty to communicate simply the characteristic passages of the original papers.

The presence of a periodical decrease and increase in the magnitude of the daily variations of the magnetic needle was stated by Lamont already in 1845\*. In the year 1851 the same philosopher published a paper† in which he deduced a time of  $10\frac{1}{3}$  years for the duration of this period. At the same time Sabine‡ was engaged in a research and comparison of the disturbances of declination in Toronto and Hobarton for the five years 1843–1848, and remarked that during this period the magnitude and frequency of the disturbances increased from year to year.

Sabine arrived also at the assumption of a period in the magnitude of these disturbances, and, passing to the consideration of a possible cause (*l. c.* p. 121), says:—

“As the sun must be recognized as at least the primary source of all magnetic variations which conform to a law of local hours, it seems not unreasonable that in the case of other variations also, whether of irregular occurrence or of longer period, we should look in the first instance to any periodical variation by which we may learn that the sun is affected to see whether any coincidence of period or epoch is traceable. Now the facts of the solar spots, as they have been recently made known to us by the assiduous and systematic labours of Schwabe, present us with phenomena which appear to indicate the existence of some periodical affection of an outer envelope (the photosphere) of the sun; and it is certainly a most striking coincidence that the period and epochs of minima and maxima which M. Schwabe has assigned to the variation of the solar spots are absolutely identical with those which have been here assigned to the magnetic variations.”

Also at the same time, and quite independently of each other, R. Wolf§ and Gautier|| had drawn attention to the coincidence in the period of sun-spots with the periodical changes in the earth's

\* Dove's *Repertorium der Physik*, vol. vii. p. 102. Compare also “Results of the Magnetical Observatory at Munich, 1843, 1844, and 1845,” *Abhandl. d. II. Classe der bayr. Acad. der Wiss.* vol. v. part 1.

† Poggendorff's *Annalen*, vol. lxxxiv. p. 572.

‡ “Periodical Laws discoverable in the mean effects of the larger Magnetic Disturbances,” by Col. Edw. Sabine, R.A. (received March 18, read May 6, 1862), *Philosophical Transactions*.

§ *Mittheilungen der Berner naturforschenden Gesellschaft*, No. 245. *Comptes Rendus*, September 13, 1852. *Astr. Nachrichten*, No. 820.

|| *Bibliothèque Universelle*, July and August 1852.

magnetism ; and the former gentleman has since succeeded, with great zeal and trouble, in establishing this relation as an undoubted fact.

In proof of this assertion I cite some passages from a paper of Wolf, "On the Eleven-years period of the Sun-spots and Magnetical Variations"\*:—

"When I showed, in the year 1852, that there exists a period of  $11\frac{1}{3}$  years in the frequency of sun-spots which could be followed backward as far as the discovery of sun-spots, I had, besides the series of Hofrath Schwabe, which already then extended over twenty-six years, nothing at my disposal but a certain number of short series of observations and separate statements; the proof therefore only lay in the two facts, that Schwabe's series showed such a period, and that the assumption of a period extending over  $11\frac{1}{3}$  years was not contradicted by any of the statements found. Now it is otherwise. The discovery of observations made by Staudacher, Flangergues, Tevel, Adams, &c. renders it possible to me to express by relative numbers the mean frequency of sun-spots for a series of 112 years as found from 20,000 observations. I give these numbers in full, adding, as far as space permits, the mean yearly variation in declination."

As regards the above-mentioned complete reproduction of the observations, I refer to the paper itself, and only give the results of Wolf in his own words (*l. c.* p. 505):—

"The above Table shows at first sight the periodical change of the frequency of sun-spots as well as in the magnitude of magnetical variations in declination. The following periods result from it:—

Sun-spots and Magnetism of the Earth.

Maximum.		Minimum.	
Sun-spots.	Magnetic variations.	Sun-spots.	Magnetic variations.
1750·0±1·0		1755·7±0·5	
1761·5±0·5		1766·5±0·5	
1770·0±0·5		1775·8±0·5	
1779·5±0·5		1784·8±0·5	1784·5±0·5
1788·5±0·5	1787·2±1·0	1798·5±0·5	1799·0±2·0
1804·0±1·0	1803·5±1·0	1810·5±0·5	
1816·8±0·5	1817·5±1·0	1823·2±0·5	1823·8±1·0
1829·5±0·5	1829·7±0·5	1833·8±0·2	
1837·2±0·5	1837·7±0·5	1844·8±0·2	1844·2±0·5
1848·6±0·5	1848·9±0·3	1856·2±0·2	1856·3±0·3
1860·2±	1860·0±0·3		

"If we consider, first, the epochs corresponding to the sun-

\* Pogg. *Ann.* vol. cxvii. p. 502 (1862).



spots, we find from the outside numbers for the length of the mean period :—

$$\frac{[1860.2 \pm 0.2] - [1750.0 \pm 1.0]}{10} = 11.02 \pm 0.10,$$

$$\frac{[1856.2 \pm 0.2] - [1755.7 \pm 0.5]}{9} = 11.17 \pm 0.06.$$

These results agree, within the limits of errors of observations, with the previously deduced period of  $11\frac{1}{9}$  years.”

“Considering, secondly, the period of magnetical variations, we shall see that it agrees with the period of sun-spots, and that the parallelism of the two appearances *has been proved the more strikingly, as not only the mean period is exactly the same, but even the anomalies of one are exactly found again in the other.*

“This agreement induced me already, more than three years ago, to express the opinion that there exists such a connexion between the two phenomena that the intensity of the common cause could be read off in both as on two different scales. It would then be possible to calculate the magnitude  $v$  of the magnetic variation from the relative number  $r$  of the year in question by a formula of the form

$$v = a + b \cdot r.$$

I found, for instance, at that time for Munich the formula

$$v' = 6'.273 + 0.051r,$$

which represented more exactly the variations of declination for the years 1835–1850 as observed by Lamont than the formula which had been found directly by himself”\*.

It must be said in conclusion that Professor R. Wolf had deduced such a formula for Prague. He remarks on its agreement with observation, in a recent publication, as follows † :—

“The value  $9'.44$ , which I had calculated in No. 26 from the sun-spots for the variation for Prague in 1869, is sensibly larger than that found by observation  $2^h-20^h$ ,  $8'.69$ ; but it agrees exactly with that flowing from the minimum and maximum values, viz.  $9'.44$ .”

A connexion which is not less astonishing is that found to exist between the frequency of the aurora borealis and that of the sun-spots. I take the epochs deduced from a great number of European and American observations by Professor Loomis in a paper published in 1865 in the ‘Annual Report of the Board

\* As to the differences of opinion between Lamont and Wolf, which, however, seem to be decided in favour of the latter, I refer to the two cited papers in Poggendorff’s *Annalen*, 1862.

† *Vierteljahrsschrift der naturforschenden Gesellschaft zu Zürich*, December 1870, p. 253.

of Regents of the Smithsonian Institution &c. for the year 1865' (pp. 208-248). This compilation is the more important, as Loomis never mentions the connexion between sun-spots and magnetic variations; the researches of Wolf were at any rate not known to him\*. Any preconceived opinion was therefore impossible, and the connexion between the two phenomena becomes the more astonishing.

In the following Table I have separated, as above, the years of sun-spot maxima and minima, and annexed, in place of the magnetic variations, the aurora-borealis epochs of maxima and minima deduced by Loomis from *European* observations only. The years deduced from American observations are almost identical with those given below.

Sun-spots and Auroræ Boreales.

Maximum.		Minimum.	
Sun-spots.	Aurora borealis.	Sun-spots.	Aurora borealis.
	1707		1713
	1718		1721
	1730		1733
	1741		1745
1750·0	1750	1755·7	1755
1761·5	1760	1766·5	1766
1770·0	1771	1775·8	1776
1779·5	1779	1784·8	1784
1788·5	1788	1798·5	1798
1804·0	1804	1810·5	1811
1816·8	1819	1823·2	1823
1829·5	1830	1833·8	1834
1837·2	1840	1844·0	1843
1848·6	1849	1856·2	1856

These observations leave no doubt as to the connexion between terrestrial magnetism and the occurrences on the solar surface; and I only add that my theory not only explains the existence but also the nature of this connexion. Indeed, if the only question were, to confirm its existence, it would not have mattered whether a maximum of sun-spots coincided with a maximum or with a minimum of magnetic disturbances; but, according to the theory developed above, the coincidence of the maxima of the two phenomena is a necessity. For at the time of maximum of sun-spots the sun's increased magnetic action will cause an acceleration in the glowing streams within the earth; and hence the magnitude of the magnetic disturbance will increase, just as it does, in higher geographical latitudes (compare § 10).

\* Only later, in Silliman's Journal, September 1870, did Loomis lay stress upon these connexions. Compare the critique of his paper by Wolf in No. 28 of his *Astronomische Mittheilungen*.

It has been shown (§ 15) that the position of the sun's axis with respect to the plane of the ecliptic causes theoretically a stronger magnetic induction near the two days of the year during which the earth is most exposed to the inductive action of one of the sun's poles, leaving out of consideration the influence which the inclination of the earth's axis must have on this induction. It is therefore supposed that on these two days the radius vector of the earth is, as in the time of the equinoxes, perpendicular to the earth's axis. As those two days have been found to be the 6th of September and 7th of March, the supposition may be considered approximately true for a rough comparison with the magnetic disturbances. According to our theory, at those times all those phenomena must have a maximum which are caused by an increased magnetic induction of the earth; such are the magnetic disturbances and the auroræ boreales.

I take as material for the examination of these consequences the *Resultate aus den Beobachtungen des magnetischen Vereins* 1836, III., which are discussed by Gauss in the fifth volume of his collected works.

Under the title "Mean Variation of the Magnetic Declination during 1834-1837," these values are given as monthly means for the three years mentioned. Although the number of observations is much too small to allow a decisive comparison with my conclusions, I still give them, expressed in seconds of arc (*l. c.* p. 567).

*Mean Variation of Declination at Göttingen from 1834 to 1837.*

Month.	Variation.
January . . . . .	189
February . . . . .	155
March . . . . .	206
April . . . . .	164
May . . . . .	196
June . . . . .	172
July . . . . .	223
August . . . . .	244
September . . . . .	204
October . . . . .	216
November . . . . .	191
December . . . . .	195

Although these numbers show a maximum in March and between August and September, agreeing with the times mentioned above, a much greater number of observations is required to be discussed from this point of view.

The frequency of the aurora borealis is also subjected to a yearly period. Loomis has deduced such a period in the paper



mentioned above from a great number of observations made at New Haven, Boston, and Canada, and extending over a range of 113 years. The comparison yields the following results:—

*Frequency of Auroræ Boreales as dependent on the time of the year.*

Month.	Number of auroræ boreales.
January . . . . .	173
February . . . . .	210
March . . . . .	240
April . . . . .	267
May . . . . .	191
June . . . . .	179
July . . . . .	244
August . . . . .	238
September . . . . .	293
October . . . . .	236
November . . . . .	215
December . . . . .	159

Here also, therefore, the maxima are in the same two months. We must, however, bear in mind that, in consequence of the great changes of the radius vector of the earth in these months, the secondary inductive actions between sun and earth must also have then a maximum, so that only by this, without regard to any influence of the position of the sun's axis, the magnetic induction would be increased in these two months.

So, for instance, in the year 1870 the greatest velocity with which the earth was going away from the sun was between the 24th and 26th of March, 498·1 metres a second. The maximum value for its approach, between the 1st and 3rd of October, was 502·4 metres a second. It is clear that, according to the laws of electrical induction, such rapid changes of the distance between the earth and the sun must cause electrical currents in these bodies, even if only one of them exerted a magnetic action upon the other. How great the intensity of these currents would have to be in order to produce auroræ boreales and magnetical disturbances on the earth's surface can only be decided by observation. It is sufficient for us to have shown the necessity of the existence of these currents without regard to their intensity.

17.

The above inquiries have shown that the consequences deduced by the theory are confirmed by facts proved by observation. All phenomena on the solar surface which are connected with changes in the streams necessarily call forth analogous

changes in the streams in our earth, and by this generate variations in the terrestrial magnetism. Until now we have, however, only considered those causes of changes in the streams of the solar surface which are produced by the periodical number of sun-spots. Let us now see whether we can find other processes on the sun, of a more accidental and local nature, which, similar to earthquakes or volcanic eruptions, could mechanically produce sudden changes in the velocity of the glowing streams. Just as the purely mechanical causes produce a disturbance in the whole magnetic force of the earth, they must have, if sufficiently intense, the same effect on the sun, and produce there changes which in their turn may be accompanied by all the phenomena observed in consequence of magnetic disturbances.

The spectroscope has discovered such a richness of mighty volcanic eruptions on the solar surface that a mechanical reaction on the glowing streams seems to be a natural supposition. Such reactions would chiefly manifest themselves at the great changes and sudden ruptures of the immense slag-like masses which we observe in the sun-spots.

The observations show indeed the very same connexion between local processes on the sun's surface as was deduced by our theory.

I give here the most remarkable of these observations known to me. They date, of course, only from the time when the solar surface was first made the subject of careful researches.

Poggendorff's *Annalen*, vol. cix. p. 190 (January 1860), contains an extract of a letter from Major-General Sabine to Professor Dove, under the title of "An Observation of Sun-spots." It runs as follows:—

"Mr. Carrington being engaged in the morning of the 1st of September last year in taking his daily observations on the form and position of the sun-spots, saw, to his great surprise, a white light which was breaking forth from the middle of a large spot which had already during several days been the subject of general attention; this white light was more intense than the rest of the solar surface. It lasted somewhat longer than five minutes; and when it had disappeared, the large spot seemed to be unchanged. This phenomenon was also seen by Mr. Hodgson at Highgate, at a distance of some miles from Red Hill, the observatory of Mr. Carrington. Both observers agree in fixing the time of appearance and disappearance (right within a few seconds) at 11<sup>h</sup> 18<sup>m</sup> and 11<sup>h</sup> 23<sup>m</sup> Greenwich mean time. A few days afterwards Mr. Carrington had occasion to visit the Meteorological Observatory in Kew; and talking about the phenomenon, he looked at the photographic records made there by the three magnetical elements. In each of them he observed a great dis-

turbance, which, as far as he could judge, had taken place simultaneously with the phenomenon observed in the sun's photosphere. This, I think, is the first example of a connexion between the physical changes in the photosphere and the magnetic storms or disturbances indicated by me in the year 1852."

Since the application of the spectroscope with a wide slit to the observation of the solar surface, and chiefly of the edges of the sun's disk, we are able to convince ourselves directly of the immense power of the eruptions; so that at the sight of these phenomena the probability of a strong reaction on the liquid surface and its currents is increased almost to conviction.

Through a friendly communication from Professor C. A. Young, Dartmouth College, America, who is in possession of excellent spectroscopic instruments which he himself has much improved by ingenious arrangements, I am in a position to illustrate here such an eruption by drawings and numerical measurements. The eruption referred to belongs probably to the most intense and powerful which have been registered during the short time of spectroscopic observations of the sun. The short description of the phenomenon with the drawings of the observer, as communicated by Professor Young to the 'Boston Journal of Chemistry,' were sent to me while this paper was being printed.

[As the description is given in the January Number of the Phil. Mag., it is not necessary to reproduce it here. *Vide supra*, pp. 76-79.]

It is to be seen from this description how great reactions take place on the solar surface, and how probable, according to my theory, the supposition is that such sudden appearances must generate magnetical changes in the sun which must reflect themselves, if sufficiently intense, in the magnetic condition of the earth\*.

If I am allowed to compare the character of the phenomena just described with terrestrial appearances, I may remind the reader of air- and water-spouts, where for a long time quietness and stability seem to reign, which suddenly collapse, betraying by mighty movements the former condition of whirl-like movements in the inner masses. Indeed the five stems in fig. 1 recalled to my mind such phenomena.

18.

These facts will be sufficient for the present to show at least the probability of the existence of a connexion between the magnetic condition of the earth and sudden changes on the solar sur-

\* During the printing I receive the news, as an answer to an inquiry directed to the Astronomer Royal (Mr. Airy), that about three hours after that explosion a magnetic storm began to rage on the earth.



face. Most likely the observations of the next few years will place this relation beyond all doubt.

We must now ask whether the moon can also have, according to my theory, a magnetic influence on the earth, and of what kind this influence must be.

The magnetic influence at a distance of the heavenly bodies is, according to the theory developed here, connected with two conditions:—

1. The existence of liquids flowing according to definite laws, either on the surface or in the interior of the bodies.

2. The bodies must have such dimensions that their size is not too small in comparison with their distance, so that an inclination of their magnetic axis may cause a difference in the action of the two poles.

The first of these conditions may be regarded as not fulfilled in the moon, if we consider its time of rotation as well as its general exterior appearance. But it is known that the second condition is the cause of the tides.

If, therefore, the moon were of a magnetic mass or of one liable to electric induction, *i. e.* if its chief constituents were good conductors, the varying position of the earth's axis with respect to the moon would necessarily have an influence upon the earth's magnetism. This influence, according to our theory, is inconceivable without a simultaneous reaction on the inner streams of the earth. It follows that the influence which the moon or any heavenly body exerts on the earth by induction cannot be so simple as the inductive actions we observe in our laboratories in *solid masses* of small dimensions. Therefore we can in general only expect to find the duration of the period which depends on the declination and hour-angle, while the times of maximum and minimum can only be found theoretically by taking account of the mechanical reaction necessarily accompanying magnetic induction.

I take the liberty of stating in the following the facts proved by the observations of reliable philosophers, and I leave to the reader to form an independent judgment as to how far these observations can be taken as confirmations of the above consequences of my theory.

As regards the historical question of the discovery of a magnetic influence of the moon upon the earth, I cite the words of Lamont\*.

“Kreil was the first who deduced from his observations an influence of the moon on the declination, and then also on the intensity; his results were confirmed by Brown, who also found an influence, which, however, was not the same in all seasons; recently Airy has occupied himself with this problem, and arrived

\* *Berliner Berichte der physikalischen Gesellschaft*, 1861, p. 558.

at series of numbers which show increase and decrease when compared with the course of the moon.

"Sabine is the only one who found the same law for all stations of observation and in different seasons."

I shall confine myself in the following to considering in detail the results of Sabine.

His first paper is entitled "On the Evidence of the existence of the Decennial Inequality in the Solar-diurnal Variations, and its non-existence in the Lunar-diurnal Variation of the Magnetic Declination at Hobarton"\*.

In this memoir the hourly observations of Toronto are divided into several periods, and the lunar influence is deduced from each of these periods. All the individual periods lead consistently to the following results:—

1. The moon manifests its influence in the variation of all the magnetic elements; its action can decidedly be shown to exist in declination, dip, and intensity.

2. The lunar influence consists of a regular period with double maximum and double minimum; the maxima are, for the declination, 6 hours and 18 hours, for dip 3 hours and 14 hours, and for intensity 3 hours and 16 hours, after the upper culmination; the magnitude of the periods (difference between maximum and minimum) is, for declination,  $0^{\circ}64$ , for inclination  $0^{\circ}07$ , for total intensity  $0^{\circ}000012$ .

3. These movements may be explained by the hypothesis that the earth induces magnetism in the moon.

4. The lunar influence does not show any trace of a decennial period.

The last-mentioned result, which constitutes a characteristic difference between the magnetic influence of sun and moon, might be considered, from what has been said at the beginning of this section, a confirmation of the conclusion that in the inner part of the moon no glowing liquid mass is present which could give rise to disturbances, or that, if these currents are present, they are not subjected to such changes as those on the solar surface by the periodically changing quantity of sun-spots.

A second paper of Sabine has the title "On the Lunar-diurnal Variation of the Magnetic Declination obtained from the Kew Photograms in the years 1858, 1859, and 1860"†.

In this paper Sabine deduces the influence of the moon from the photographically registered observations of Kew, and shows that there exists a regular period with two maxima and two

\* Proceedings of the Royal Society, vol. viii. p. 314 (1857); Phil. Trans. 1857, pp. 1-9.

† Proceedings of the Royal Society, vol. xi. pp. 73-80. Phil. Mag. 1861, Ser. 4, vol. xxii. p. 479-485.

minima (analogous to flux and reflux); the numbers deduced for the different years show a remarkable consistency. On the other hand, Sabine proves that the movements at Kew correspond exactly to those at Hobarton, with the exception that in the two opposite hemispheres they are in the inverse direction; that is to say, the north end of needle in the northern hemisphere moves as the south end does in the southern hemisphere. The difference in the magnitude of the movement is easily explained if we consider the difference in horizontal intensity at the two places. In Kew this intensity is 3·7, and in Hobarton 4·5 (absolute English units). Sabine does not make any supposition in order to explain the difference in sign, but only remarks that "we may assume either a direct influence, *i. e.* an attraction of the needle by the moon, or an indirect influence, *i. e.* a magnetization of the earth's nucleus by the moon:" these are the words of Lamont referring to the contents of another paper by Sabine.

The circumstance which is urged as not explained, that the form of the magnetic influence is different in the two hemispheres, is, according to my theory, as shown above, a physical necessity.

I only state these facts, which confirm that the magnetic influence of the moon is just as we expected it to be from our theory, and will not give any opinion whether the change in the magnetic constants which was recently observed at the beginning of the totality of a solar eclipse is caused by the increased magnetic action of sun and moon, or rather by a mechanical influence in the form of a tidal or pressure-wave in the glowing liquid nucleus of the earth.

My opinion of a mechanical reaction on the glowing streams in the earth which accompanies every magnetic induction and the complication of the phenomena caused by this will be supported by a theoretical research of Lloyd, by which he proves that the influences of sun and moon are rather caused by an indirect than by a direct influence on the earth.

The paper bears the title\* "On the direct Magnetic Influence of a distant Luminary upon the Diurnal Variations of the Magnetic Force at the Earth's Surface. By the Rev. H. Lloyd."

In the introductory words the author remarks:—

"It has been usual to ascribe the ordinary diurnal variations of the terrestrial magnetic force to solar heat, either operating directly upon the magnetism of the earth, or generating thermoelectric currents in the crust. The credit of these hypotheses has been somewhat weakened by the discovery of a variation which is certainly independent of any such cause, namely the lunar variation of the three magnetic elements; while at the

\* Phil. Mag. S. 4. vol. xv. pp. 193–196 (1858).



same time new laws of the solar-diurnal change have been established, which are deemed to be incompatible with the supposition of a thermic agency. There has been, accordingly, a tendency of late to recur to the hypothesis that the sun and moon are themselves endued with magnetism, whether inherent or induced; and it is therefore of some importance to determine the effects which such bodies would produce at the earth's surface, and to compare them with those actually observed.

"I have endeavoured, in what follows, to solve this question on the assumption that the supposed magnetism of these luminaries is inherent. The result will show the insufficiency of the hypothesis to explain the phenomena—and will therefore bring us one step nearer to their explanation, by the removal of one of their supposed causes."

The results of the analytical research which follows are summed up by the author in the following words:—

"From the foregoing we learn:—

"1. That the effect of a distant magnetic body on each of the three elements of the earth's magnetic force consists of two parts, one of which is constant throughout the day, while the other varies with the hour-angle of the luminary.

"2. Each of these parts varies inversely as the cube of the distance of the magnetic body.

"3. The variable part will give rise to a diurnal inequality, having one maximum and one minimum in the day, and subject to the condition

$$\Delta_{\theta} + \Delta_{\pi+\theta} = 0.$$

"The third of these laws does not hold, with respect either to the solar or to the lunar-diurnal variation. Thus, in the solar-diurnal variation of the declination, the changes of position of the magnet throughout the night are comparatively small, and do not correspond, with change of sign only (as required by the foregoing law), to those which take place at the homonymous hours of the day.

"The phenomena of the lunar-diurnal variation are even more opposed to the foregoing law, the variation having two maxima and two minima of nearly equal magnitude in the twenty-four lunar hours, and its values at homonymous hours having for the most part the same sign. Hence the phenomena of the diurnal variation are not caused by the direct magnetic action of the sun and moon."

In the discussion of this paper\*, Lamont agrees perfectly with these results, and concludes his account with the following words:—

"From this Mr. Lloyd draws justly the conclusion that the

\* *Berliner Berichte der phys. Ges.*, 1858, p. 592.

daily variations of the earth's magnetism cannot be accounted for by a direct magnetic influence of sun and moon."

I believe I can show still clearer by the following the insufficiency of the supposition of a direct magnetic influence of sun and moon as the only cause of the action of these bodies.

By whatever cause a heavenly body, as for instance the sun, acquires magnetic polarity, we must think it probable, according to the phenomena presented to us by earthly bodies, that the potentials of the two opposite and separated magnetisms are equally great. If the sun possessed two magnetic poles, the earth in its yearly course round the sun would necessarily come twice into a position in which the magnetic induction of the two poles would destroy each other. At these two days of the year the daily variation would disappear if it were only produced by direct magnetic induction. We have here supposed that the two magnetic poles coincide with the geographical poles. But if, as in the earth, this is not the case\*, and if the mutual distance and the intensity of the two magnetic poles are sufficiently great compared with their distance from the earth, the daily variations, if only produced by the changes of position of these two poles by the sun's rotation, would necessarily disappear about every thirteen days, and then reach a positive or negative maximum corresponding to the induction of the sun's north or south pole. By this consideration, therefore, we obtain an empirical criterion for deciding the question whether the sun exerts an appreciable magnetic induction upon the earth, and whether the daily variations are only produced by this induction, or whether other causes come into play.

## 19.

Bearing in mind the magnetic influence of the moon which we have discussed above, it is clearly the most probable supposition that the sun exerts a double influence—a direct magnetical one, and, secondly, as in the case of the moon, a dynamical one, through the generation of a wave of pressure in the glowing liquid nucleus of the earth. This dynamical influence alone would, according to my theory, be sufficient to produce a variation of the magnetic constants. It is to be seen, from the difference pointed out by Lloyd between the daily periods of the sun and moon, that the latter bear the character of a tidal wave with its two maxima and minima in one day.

Mr. Carl Hornstein, the Director of the observatory in Prague, in a paper "On the Dependence of Terrestrial Magnetism upon the Sun's Rotation," has confirmed the fact that the sun exerts

\* On the necessity of this non-coincidence, and its physical cause, compare what has been said above, § 8.

an influence of the above kind by the rotation of its magnetic poles.

Hornstein justly takes for the time of rotation of the sun that of the equatorial zone as found by the observations of Carrington and Spörer. For I have shown, in my paper "On the Law of the Sun's Rotation," that the curious difference in the times of rotation for different heliographic latitudes is only generated by the friction against the polar undercurrents; and it is therefore clear that even the equatorial part of the sun rotates in a time slightly different from that of the inner nucleus, with which time the magnetic poles are connected.

Hornstein says, speaking of the difference of rotation for different heliographic latitudes:—

"If we are unwilling to make suppositions which are in contradiction with the fundamental laws of mechanics, we must assume the time of rotation of the sun-spots at the equator (24·541 days), or one very near it, to approach nearest to the real time of rotation of the sun."

Then Hornstein explains the reasons which first led him to the idea that possibly the magnetic variations on the earth might show a dependence upon the time of rotation of the sun.

The words of Hornstein are the following:—

"It has been, as is known, observed several times during the last few years that exceptional changes on the sun's surface happened simultaneously with great changes of the direction and force of the earth's magnetism. At the same time it has been proved, by the important researches of Sabine, Wolf, Lamont, and others, that the mean daily variations of magnetic declination show the same period of eleven years as the sun-spots, and that the same thing holds for the variation of horizontal intensity. This is a proof that great changes on the sun's surface (which are most likely nothing but great revolutions in the sun's nucleus) may cause changes in the elements of the magnetic force of the earth.

"Different conditions on the surface of the sun do not only take place in the course of the eleven-year period of the sun-spots, they are present at the same time and by the side of each other, if we look at regions of different heliographical longitudes of the sun-spot zone. As by the rotation of the sun all these regions are turned one after the other towards the earth, and as during this period every point of the zone mentioned changes its distance from the earth nearly by an entire diameter of the sun, I came to the idea to look whether I could not find periodical changes in the elements of the earth's magnetism, the period of which is equal to the synodic\* time of rotation of the sun, or any aliquot part thereof.

\* That is, with regard to the earth's movement, the time required for  
*Phil. Mag. S. 4. Vol. 43. No. 288. June 1872.* 2 H



"I have extended my researches over all three elements; and from the discussion of the observations made during several years at Prague, Vienna, and other places, it follows that the changes of each of the three elements of the constant force indicate a period of about  $26\frac{1}{3}$  days,—a periodicity which can be scarcely explained otherwise than by the action of the sun."

Hornstein next explains the method he has pursued in discussing the observations, and continues (p. 10) with the following words:—

"The existence of an oscillation taking place in nearly twenty-six days has therefore been proved almost beyond doubt; and I do not hesitate to regard it as an action of the sun. I was therefore obliged to take into account the above-mentioned great irregularity, which is shown in all periodical phenomena of the sun and the phenomena connected with it. Indeed the mode of proceeding which I have pursued, and which would give the more exact results the more regular and continuous the periodical appearances are, would only yield moderate results if we were, for instance, to employ it for the more accurate determination of the eleven-year period of the sun-spots. I therefore first inquired whether I was justified in assuming that the mean condition of the sun during a series of rotations is constant enough to give accurate results. I have therefore separated the observations for Prague extending from the 19th of April to the end of August 1870, and containing five periods, from the observations of September 1, 1870, to the beginning of 1871 (containing four periods). Each of these two sets of observations was treated graphically. I arrived at the following numbers for the oscillation:—

(April to August)  $0.8 \sin (x + 90^\circ)$ ;  $x=0$  on the 6th of May.  
(Sept. to Dec.)  $0.8 \sin (x + 90^\circ)$ ;  $x=0$  on the 14th of Sept.

The amplitude therefore remained constant during many months. The period, however, found in this way is somewhat shorter, viz.  $T=26.20$  days. This result is in part favourable to the assumption of the constancy of the sun's condition during a longer period.

"The calculation of the periodic course of the declination for later or earlier years with the aid of the results of 1870 would be more decisive. In order to arrive at this I first derived a mean value for  $T$ . I found,

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each point of the sun to come to the same position with respect to the earth. As the movement of the latter corresponds with the direction of rotation of the sun, the synodic time of revolution is of course greater than the sidereal or absolute one. Taking the above value for the absolute time of rotation, the synodic time would be about  $26.33$  days.

From the declination at Prague in 1870 (calculated)	T=26·69
„ „ „ „ (graphically)	„ 26·20
„ „ „ Vienna in 1870 (calculated)	„ 26·39
„ dip at Prague in 1870 . . (calculated)	„ 26·03

The mean value,

$$T=26·33 \text{ days,}$$

may be regarded as the most probable duration of the period, and hence as the result of the first attempts to calculate the synodical time of rotation of the sun by the aid of the magnetic needle.

“The true time of rotation of the sun is found from this to be =24·55 days, or almost exactly the same as the time of revolution of the sun-spots near the equator as found by the astronomical observations of Spörer.

“I have represented graphically, and investigated with reference to the  $26\frac{1}{3}$ -day period, a series of magnetical observations at Prague, Vienna, Kremsmünster, Dublin, Toronto, St. Helena, &c., extending over several years; and I have partly subjected them to calculation.”

The two Plates which accompany the paper, and cannot here be reproduced, show graphically the periodicity mentioned. To the values of a dotted line constructed by means of the mean monthly values of declination Hornstein added the periodical oscillation, and joined the points so found by a red line. He remarks as follows with reference to this line:—

“In this way the red line was drawn, which (with unimportant exceptions at the end of 1869) is in harmony with the real course of the declination as far back as the beginning of 1869, or twenty full rotations of the sun before the middle of 1870. This is a confirmation of the supposition that through many rotations the sun was in a state of quietness, in consequence of which, in spite of great revolutions, the same part exerted always about the same influence.”

I have taken the liberty to give in detail this important and, for the understanding of the magnetic relations existing between sun and earth, most valuable research of Hornstein, because, besides its general importance, it is a proof for the views here developed on the physical origin of the magnetic action of the heavenly bodies.

It is, however, easily shown that the occurrences on the sun's surface which Hornstein mentions at the beginning of his paper *cannot* be the cause of the discovered periodicity; for, besides the difficulty, admitted by himself, of thereby explaining the constancy of the magnetic action of a definite point of the sun's surface, there are many reasons against it.

Suppose we had a magnet at the mean distance of the earth from the sun, the length of which was equal to the earth's diameter. In the most favourable condition a mass able to be magnetized on the sun's surface could only be  $\frac{1}{11696}$  nearer to one end of the needle than to the other. The variation of this fraction in consequence of the sun's rotation could not be perceptible, even without regard to the fact that any local magnetic phenomenon on the sun must be accompanied by both magnetisms, which would destroy each other in consequence of the small difference in distance at which they are from the earth.

The conditions will be found to be quite different if we consider the sun itself to be a large magnet which, as it does not coincide with the axis of rotation, is subjected to a mutation, just as the magnetic axis of the earth, in consequence of the rotation of the heavenly body. Supposing, for instance, the magnetic poles of the sun had heliographic latitudes of  $70^\circ$ , similar to that of the magnetic north pole of the earth, the variation produced by their rotation would amount to about one per cent of the total magnetic force exerted by the sun upon the earth. At the same time the constancy in the situation of the magnetically active poles, which is discussed by Hornstein, is found to be a physical necessity under this supposition (which, according to my theory, is a consequence of the premises made).

If the sun, in consequence of the continually unequal distances of its two magnetic poles from the earth, acts magnetically on the earth, it is clear that the mean inclination of the magnetic axis of the earth must have a considerable influence over the strength of the induction. It will therefore depend upon the position as well as relative intensity of the earth's poles (perhaps also upon the secondary influences of heating and cooling in summer and winter) what modifications the periodical variations will undergo in the course of the day or the year.

It is self-evident that, according to my theory, the distribution of heat and cold on the earth's surface must have an influence on the glowing streams within the earth. Whether we can perceive this influence in the magnetic variations caused by it, depends on its intensity, and cannot be decided *à priori*, but only empirically. There are, however, some observations which I think can only be explained by an influence of the distribution of temperature, or of the inner configuration of the earth's crust.

Sabine\*, for instance, has found in his magnetic observations at Spitzbergen that the influence of the sun's position upon the daily variations of declination is much more decided and regular than at other places. On this subject the reporter in Gehler's *Wörterbuch* (vol. vi. p. 1097) says:—

\* An account of Experiments to determine the Figure of the Earth (4th ed., London, 1825), p. 500.



"It is remarkable that in this place, where the unequal influence of land and water disappears, as the whole surrounding parts form a nearly continuous cover of ice, the variation exactly coincides with the course of the sun. This would be an important argument for the derivation of the magnetism from the sun's rays."

The results of Lenz\* point to a similar relation of a permanent distribution of temperature on the surface of the earth to the inner movement in the glowing liquid mass. The paper of Lenz bearing on this question was published under the title of "Investigation of an Irregular Distribution of the Earth's Magnetism in the northern portion of the Gulf of Finland."

In a report, Lamont says concerning the results of this research:—

"The paper of Lenz, announced in the *Berliner Berichten*, 1860, p. 654, on the local disturbances at the entrance of the Gulf of Finland, has now appeared, and contains exact information about the instruments and methods of reduction, the immediate results of observations, and the values calculated therefrom for the three rectangular magnetic components. The result is that we have to consider the island of Jussar-oe a great natural magnet, whose north pole is to the north-west and south pole to the south-east. By this hypothesis, however, the phenomena are only explained in their general character; and if we go into details, we shall find that the magnetism is irregularly distributed over the island itself; and the abnormal distribution is not confined to the island and its immediate neighbourhood, but extends over a wide area. It would be of interest to continue this research still further, and especially to begin observations on the south coast of the Gulf, where the existence of a local disturbance has been shown to exist. M. Lenz seeks the origin of the disturbance in the huge beds of ice which are found in that place."

[To be continued.]

#### LVIII. Notices respecting New Books.

*A Treatise on the Theory of Friction.* By JOHN H. JELLETT, B.D., Senior Fellow of Trinity College, Dublin; President of the Royal Irish Academy. Dublin: Hodges, Foster, and Co. London: Macmillan and Co. 1872. (8vo. pp. 220.)

IN all cases of equilibrium, where a body rests against one or more fixed surfaces, it is usual to assume that the reaction of each fixed surface takes place along the normal at the point of contact. By this assumption an approximate solution is obtained of many questions which become much more difficult, or even insoluble, if a closer

\* *Mém. de l'Acad. de St. Pétersbourg*, vol. iii. pp. 1-38.

approximation to their real conditions is attempted. In all actual cases, however, surfaces are capable of exerting a tangential as well as a normal reaction; and the conditions of these cases are much more closely represented if it is assumed that, as well as a normal reaction ( $R$ ), there is also a friction or tangential resistance ( $F$ ) which acts in any direction needed to oppose sliding, and of any amount needed for equilibrium up to a limiting value  $\mu R$ , where  $\mu$ , the coefficient of friction, has a definite numerical value depending on the surfaces of contact. The object of Mr. Jellett's Treatise is to discuss the properties and effects of this force as a part of rational mechanics, and not merely to consider it as a force by reason of which certain corrections have to be applied to results obtained on the supposition of perfect smoothness before those results are capable of useful application (p. v),—in other words, to trace out the consequences that follow from the assumption that the reactions follow this law absolutely.

The contrast between the usual and the more exact assumptions can be made as follows:—Suppose a point acted on by any forces to rest against a plane; let  $Q$  be the resultant of all the forces, the reaction of the plane excepted; let a cone be described round the normal as axis, with the point as vertex, and semi-vertical angle equal to the angle of friction ( $\tan^{-1} \mu$ ). If the plane were smooth, the condition of equilibrium would be that  $Q$  act along the normal; but if the plane is rough,  $Q$  will be balanced by the reaction of the plane, provided it act along any line within the cone. If  $Q$  act along a line on the surface of the cone, the point is in an extreme position, *i. e.* it is on the point of sliding\*. This does not seem a very serious modification of the conditions; but when its consequences are worked out in any particular case they frequently show the problem in an entirely new light. Speaking generally, the effect is to introduce into the solution quantities which are indeterminate and limited by one or more inequalities, instead of quantities which would admit of exact determination if the surfaces were smooth. The solution, however, ordinarily becomes determinate if we suppose it to be made with reference to an extreme position; but even then the solution is generally quite different from what would be obtained on the supposition of smoothness. Accordingly, in chap. ii. and iii., Mr. Jellett treats the subject under two heads: he discusses (1) the conditions to be fulfilled when equilibrium exists, and (2) the conditions under which bodies are in an extreme position.

Passing from the case of equilibrium to that of motion, the following points are discussed in chap. iv. and v.:—the motion of a particle and system of particles on rough lines and surfaces, that of a solid body on a rough plane, and the initial motion of a system of particles, and of a solid body on rough surfaces. Indeterminate-

\* This geometrical conception is due to the late Canon Moseley, an author who many years ago strongly insisted on the need of taking account of friction in mechanical questions, and exemplified his views in a very elaborate discussion of the Theory of Machines which forms part of his 'Mechanical Principles of Engineering.'

ness meets us in questions of motion as well as in questions of equilibrium, *e. g.* in the case of the initial motion of a system of particles, Mr. Jellett points out that, "it is easily shown by the principles of ordinary dynamics that, if the supporting surfaces be smooth, the initial motion and all subsequent motions are perfectly determinate;" and when the surfaces are rough, "the equations of the problem enable us to determine completely the condition of the moving particles, if we know that they *are* moving. But as these equations do not give us any means of deciding *which* particles are at rest and *which* are in motion, the initial motion of the system . . . remains still indeterminate." The amount of this indeterminateness can be reduced by rejecting certain systems of movements; but when all "have been rejected except such as are both geometrically and dynamically possible, it will be frequently found that the question remains still indeterminate; *e. g.* this will be in general the case when there exists an equation of condition involving only the coordinates of quiescent particles" (pp. 104, 105, 109).

The sixth chapter is devoted to the question of *necessary* and *possible* equilibrium—a distinction to which, we believe, Mr. Jellett has been the first to draw attention; what is meant by it will be best understood by considering a particular case:—Suppose O to be a point in front of a vertical wall; a weightless rod is fixed by one end to O and can turn freely round it; it carries at the other end a heavy point P, which is placed against the wall. If the rod were allowed to turn, the locus of P on the wall would be a circle; we will suppose C to be the centre of this circle, and ACB to be a vertical diameter, the point A at the top; suppose P placed in some position near A, and let  $\theta$  and  $\beta$  denote the angles PCA and POC respectively, and Q the reaction transmitted along the rod from O to P. Now if we suppose  $\theta_1$  to denote an angle such that  $\tan \theta_1 = \mu \cotan \beta$ , then if  $\theta < \theta_1$  and a small motion is communicated to P in this position, the forces will tend to destroy this motion, and the position is one of *necessary* equilibrium. Next let the question be regarded thus:—By resolution of forces it is shown that the normal pressure on the wall is  $Q \cos \beta$ , and that the square of the resolved pressure on the plane is

$$g^2 - 2gQ \sin \beta \cos \theta + Q^2 \sin^2 \beta;$$

so that there will be equilibrium, provided

$$g^2 - 2gQ \sin \beta \cos \theta + Q^2 \sin^2 \beta = \text{or} < \mu^2 Q^2 \cos^2 \beta.$$

This condition cannot be fulfilled for any value of Q whatsoever if  $\sin \theta > \mu \cotan \beta$ ; there is therefore a limiting value  $\theta_2$  such that  $\sin \theta_2 = \mu \cotan \beta$ ; and if  $\theta < \theta_2$ , the above condition will be fulfilled provided Q have the proper value; but as Q is quite indeterminate, such a position is one of *possible* equilibrium. We arrive, therefore, at the following result, that when  $\theta < \theta_1$ , there is *necessarily* equilibrium, and the equilibrium is stable in the sense that it will not cease if a small velocity be communicated to P; but if  $\theta > \theta_1$  and  $< \theta_2$ , there will be equilibrium or not, according to the actual value of Q, and this equilibrium, if it exist, will be unstable.



It need hardly be said that this indeterminateness is a consequence of the conditions assumed for the purpose of simplifying the conditions of the questions; it does not exist in nature. Attention is frequently drawn to this point in the course of the work; and a short chapter (chap. vii.) is devoted to its elucidation. A considerable number of particular cases are worked out in illustration of the general theorems; and in the last chapter the problems of the top, of friction-wheels, and of the driving-wheels of locomotive engines are considered in detail.

In concluding our notice of this work we must not fail to add that it seems to us one of conspicuous originality and power; it is plainly the product of long and mature thought; and it claims, and we doubt not will receive, the careful study of all who are interested in the science of Theoretical Mechanics.

### LIX. *Proceedings of Learned Societies.*

#### ROYAL SOCIETY.

[Continued from p. 396.]

February 8, 1872.—George Biddell Airy, C.B., President in the Chair.

THE following communications were read:—

“Experiments on the directive power of large Steel Magnets, of Bars of Magnetized Soft Iron, and of Galvanic Coils, in their action on external small Magnets.” By George Biddell Airy, Astronomer Royal, C.B., P.R.S.

The author, after adverting to some imperfect experiments made by Coulomb in the last century, describes the apparatus which he had himself used. He employed a bar-magnet 14 inches in length, placed in one series with its edge towards the small compass on which its directive power was estimated, and in another series with its flat side towards the small compass; also a galvanic coil 13·4 inches in length, animated by a battery of three cells, and the same coil with the insertion of a soft iron coil. In the field of experiment the earth’s magnetism was sensibly neutralized by external large magnets. The direction of the needle of the small compass was estimated by eye. The magnitude of the directive force was found by observing the position taken by the needle when the poles of a horseshoe-magnet were placed in a definite position above it: for the measure of the force of the galvanic coil without core, a very small magnet was used in the same manner; its power was found to be about  $\frac{1}{120}$  that of the horseshoe-magnet. The circle on which the deflections were observed was graduated to cotangents, which gave immediately the measure of the force of the large magnet or coil, &c. In each case, observations were taken in 30 stations in one oval ring surrounding the magnet &c., and in 38 stations in another oval ring surrounding it at a greater distance. Omitting notice of the measures in general, the following specific points are remarked:—

At a constant distance from the steel, the greatest force exerted by a magnet is not the longitudinal force at the end, but the trans-

versal force near the end. In going round the magnet there are six maxima and six minima of force.

The law of attraction of the core of a galvanic coil is not very different from that of a magnet.

The force produced by the core within the coil is very much greater than that produced by the coil alone. In some positions of the small compass it is about forty times as great, and in some about 170 times as great.

The law of force at different parts of the coil differs greatly from that at corresponding parts of the magnet or core. In the coil it is, proportionally, far greater at the end, and its direction is different. Near the end of the magnet or core the directions of force converge to a point within it, distant from the end by about  $\frac{1}{12}$  part of its length. Near the end of the coil, the directions of force converge to a point as exactly as possible at the centre of the end of the coil.

The author then describes the graphic process by which he has resolved the entire magnetic forces into constituent parts in the directions longitudinal and transversal to the magnet at every station, and gives tabular statements of the magnitudes of those constituent parts. A comparison is made with the results of an assumed law, but no satisfactory agreement is obtained.

An Appendix is subjoined, containing an investigation by James Stuart, Esq., of the theoretical attraction of a galvanic coil upon a small mass of magnetism,<sup>†</sup> and a tabular comparison of the numerical values obtained from this investigation with the numerical values found by experiment. The agreement is satisfactory.

“On a mode of Measuring the Internal Resistance of a Multiple Battery by adjusting the Galvanometer to Zero.” B. M. Jules Raynaud.

The author points out that the method given by Mr. Henry Mance for this purpose, and described in vol. xix. of the ‘Proceedings of the Royal Society’ (p. 252)\*, is identical with that which he had himself previously given, and which is described in the ‘Comptes Rendus’ for July 22, 1867; at least the only difference is that M. Raynaud prescribes putting the poles in connexion with the earth, which of course is not necessary.

*LX. Intelligence and Miscellaneous Articles.*

ON THE ABSORPTION-SPECTRA OF THE VAPOURS OF SELENIUM, PROTOCHLORIDE AND BROMIDE OF SELENIUM, TELLURIUM, PROTOCHLORIDE AND PROTOBROMIDE OF TELLURIUM, PROTOBROMIDE OF IODINE, AND ALIZARINE. BY D. GERNEZ.

I RECENTLY announced to the Academy† that the property of giving, by interposition, systems of dark lines in continuous luminous spectra, far from being exhibited exceptionally by a few substances, is found again in a number of more or less coloured vapours, in which I have been able to observe them by operating on a suffi-

\* Phil. Mag. S. 4. vol. xli. p. 318.

† *Comptes Rendus*, vol. lxxiv. pp. 660 & 803.

cient thickness of the vapours raised to a suitable temperature. By the experiments the results of which I am about to indicate, eight substances are added to the list of vapours which produce an absorption-spectrum.

*Selenium*, heated to about  $700^{\circ}$  C., gives a vapour of which a few centimetres thickness is reddish, and the tint becomes more red as the thickness increases. A stratum 25 centims. thick absorbs all the rays of the spectrum as far as the red region near the place occupied by the line *c* of the solar spectrum. When we make the experiment with a porcelain tube closed at its two ends by parallel plates of glass, and gradually heated by a row of gas-jets, during the whole period of the heating we observe only a progressive extinction of all the regions of the spectrum, starting from the most refrangible rays, as far as the red, without any trace of dark lines; but if we continue to raise the temperature, the tint of the more expanded vapour brightens, and the different regions of the spectrum reappear, furrowed with groups of black bands in the blue and the violet. The appearance has a certain resemblance to the absorption-spectrum of selenious acid which I recently described; but it is not due to the accidental production of that substance, as I have assured myself by always heating the selenium in an atmosphere of carbonic acid carefully dried, which does not produce any visible trace of selenious acid.

*Protochloride of selenium*, obtained by bringing dry chlorine upon an excess of selenium, is a brown limpid liquid, the vapour of which furrows the spectrum with lines which commence at the boundary between the green and the blue, and extend as far as the extremity of the violet.

*Bromide of selenium* exercises its absorbent properties in a different region of the spectrum. It produces systems of nearly equidistant lines when observed, like the protochloride, in a thickness of 10 centims.

*Tellurium* is more favourable than the preceding substances for the observation of the phenomenon. Heated in a tube of green glass of 2 or 3 centims. diameter, previously filled with dry carbonic acid, at a temperature near that at which the glass begins to melt, it emits a golden-yellow vapour, which produces a very brilliant absorption-spectrum, much more extended towards the red than those of sulphur and selenium, and composed of systems of fine lines spreading out from the yellow as far as into the violet.

*Protochloride of tellurium* was prepared by the action of dry chlorine on tellurium contained in a narrow tube. It forms a black mass fusible into a red liquid, which is reduced into a yellow vapour that acts very vigorously upon light. One centim. thickness is sufficient for the observation of the absorption-spectrum of this substance, which is peculiarly developed in the orange and the green.

*Protobromide of tellurium* is obtained easily by the action of bromine on an excess of tellurium. It is a crystallized substance which, by the action of heat, emits a violet vapour that gives an absorption-spectrum the most remarkable lines of which are in the red and the yellow.



*Protobromide of iodine* is a solid which may be obtained crystallized by sublimation. At the ordinary temperature it emits a vapour a slight thickness of which is of a copper-red colour, while 80 centims. thickness has a currant-red tint. The absorption-spectrum of this vapour, of the same kind as those of iodine and bromine, consists of very fine lines situated in the red, the yellow, and the orange. It differs from the effect observed when the light is passed through successive layers of the vapours of iodine and bromine.

Volatile organic matters can give, like the other vapours, absorption-spectra. Thus dry *alizarine*, heated with care, emits vapours which produce, in the middle region of the spectrum, systems of sensibly equidistant lines.—*Comptes Rendus de l'Acad. des Sciences*, April 29, 1872, p. 1190.

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DEMAGNETIZATION OF ELECTROMAGNETS. BY ROBERT W. WILLSON, JUNIOR CLASS, HARV. COLL.

Wiedemann has shown (*Pogg. Ann.* vol. c. p. 235, *Ann. de Min.* (3) vol. l. p. 189) that the intensity of the current necessary to demagnetize a steel magnet is much less than that of the current by which the bar was originally magnetized.

It has occurred to me to experiment with a view to ascertaining how far this principle can be applied to electromagnets.

The apparatus used consisted of a cylindrical bar of soft iron 8 centims. in length and 1 centim. in diameter, slightly rounded at the end. The armature was a piece of soft iron of the same diameter and 2 centims. in length.

Around this core were placed two concentric helices, wound in opposite directions, through each of which could be passed the current of a single Grove's cell.

The method of experimenting was as follows. A current was first passed through the inner helix, which, for convenience, I shall call A, and the weight supported by the bar was noted. This current being broken, the outer helix, B, was introduced into the circuit, and the corresponding weight noted. The current through B being then broken, and that through A closed, after a short time the current was again passed through B and the weight noted. The following Table shows the results :—

Wt. supported by		
A.	B.	A diminished by B.
490 grms.	250	130
470	240	130
460	230	130
460	220	120
420	220	130
410	200	110
Mean .. 452	227	125

Taking the mean result, we see that while the helix B can only develop sufficient magnetism in the bar to render it capable of sus-

taining 227 grms., yet, if it act in opposition to A, it can diminish the weight which the latter supports by 327 grms.; that is, its power to demagnetize is greater than its power to magnetize.

If, then, we suppose the coercive force of the steel bar used in Wiedemann's experiments to be represented by the helix A, and the demagnetizing current of feeble intensity to be represented by the helix B, of less magnetizing power, we have here an interesting confirmation of Wiedemann's results; while we may also extend the application of the principle to electromagnets, and may assert, in general, that a current of given intensity, or a helix of given dimensions traversed by a constant current, has greater power to demagnetize than to magnetize. It is evident that the case of demagnetizing an electromagnet by a current is more difficult than the process of demagnetizing a steel bar; for whereas in a steel magnet the resistance is simply the coercive force of the steel, so that the bar when partially deprived of its magnetism has no tendency to return to its original state, even if the demagnetizing current be broken, in the case of electromagnets the helix, by which the bar was originally magnetized, is still acting with its full power when the demagnetizing helix is introduced into the circuit.

A natural inquiry was this: if the bar were magnetized by the weaker helix B, and then demagnetized by the helix A, what additional amount of magnetism could A impart to the bar? It is evident that this case, though somewhat similar to the former, is not identical with it; here the helix B acts as resistance to the magnetization of the bar, while in the former case it acted to deprive the bar of a portion of the magnetism which it already possessed.

Accordingly the bar being magnetized by B, was submitted to the action of A, and the weight supported being noted, the reverse operation was performed, and the weights supported compared as follows:—

A demagnetized by B.	B demagnetized by A.
85	65
70	65
65	60
65	50

It will be noticed that the results in the left hand are larger than those in the right-hand column; that is, when A is simply demagnetized by B, it can support a greater weight than when it is employed to magnetize the bar against the resistance of B.

In short, the result of my experiments has been to show that a given helix, traversed by a given current, has more power to demagnetize than to magnetize, while its power to prevent magnetization is greater than either.—Silliman's *American Journal* for May 1872.

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#### THE SOURCE OF THE SOLAR HEAT. BY MAXWELL HALL, B.A.

Let us suppose that the mass of the sun is slowly but continually contracting; then, in consequence of the enormous mass subjected to this contraction, an enormous amount of heat will be developed, and it will be found that the rate or amount of contraction neces-

sary to produce the amount of heat radiated by the sun into space is so remarkably small, that ages must elapse before the effect of this contraction can become visible to us at our comparatively great distance from the sun.

In order to show that this is the case, let one foot and one second be taken as the units of space and time, and suppose that each unit of volume of the sun's mass contracts by the same amount in the same time, so that if  $z_0$  be the linear contraction of the sun's radius  $r_0$  in one second, and if  $z$  be the contraction of any other length  $r$ , measured from the centre and for the same duration of time, then

$\frac{z}{z_0} = \frac{r}{r_0}$ . The effect of this contraction may thus be compared to a

series of intermittent pulsations, acting throughout the whole of the mass and tending to diminish the volume. Let  $g_0$  be the force of gravity at the surface of the sun, and let  $g$  be the force of gravity at any point within the sun's mass considered homogeneous, whose

distance is  $r$  from the centre; then  $\frac{g}{g_0} = \frac{r}{r_0}$ . Again, let  $\rho$  be the

mean density of the sun's mass, so that the weight of any thin concentric shell, whose radius is  $r$  and thickness  $\delta r$ , will be  $4\pi g \rho r^2 \delta r$ ; and since every unit of mass in this shell falls through  $z$  feet towards the centre in a second of time,  $4\pi g \rho z r^2 \delta r$  will be the kinetic energy generated and destroyed every second by this shell alone;

and therefore  $\int_0^{r_0} 4\pi g z r^2 dr$  will be the whole kinetic energy destroyed

every second of time; and we proceed to find the corresponding amount of heat evolved.

Now

$$\int_0^{r_0} 4\pi g \rho z r^2 dr = \frac{4\pi g_0 \rho z_0}{r_0^2} \int_0^{r_0} r^4 dr = \frac{4}{5} \pi g_0 \rho z_0 r_0^3;$$

and this is the kinetic energy destroyed by the fall of a weight  $\frac{4}{5} \pi g_0 \rho r_0^3$  through a height of  $z_0$  feet, or by the fall of a weight

$\frac{4\pi g_0 \rho z_0 r_0^3}{5 \times 1390}$  through a height of 1390 feet; but the fall of one pound

avoirdupois through a height of 1390 feet generates sufficient heat to raise one pound of water through  $1^\circ$  Centigrade, or it generates

one thermal unit; hence by expressing  $\frac{4\pi g_0 \rho z_0 r_0^3}{5 \times 1390}$  in foot-pounds we

shall get the number of thermal units generated every second of time. Now  $g_0 \rho$  is the weight of a cubic foot of the sun's mass at the surface, and since

$g_0 = 27.20$  times the force of terrestrial gravity, and

$\rho = 1.43$  times the density of water, therefore

$g_0 \rho = 27.20 \times 1.43$  times the weight of a cubic foot of water at the surface of the earth. But a cubic foot of water weighs 62.5 pounds, so that  $g_0 \rho = 2431$  pounds.

Therefore  $\frac{4\pi z_0 r_0^3 \times 2431}{5 \times 1390}$ , when both  $r_0$  and  $z_0$  are expressed in



feet, will give us the number of thermal units generated every second of time.

Now it has been found by observation that the heat emitted by the sun to the earth is sufficient to melt a sheet of ice whose thickness is 0.01093 inch when exposed perpendicularly to the solar rays for one minute (Sir John Herschel, 'Meteorology'), due allowance having been made for the heat absorbed by the atmosphere; and therefore in one second a sheet of ice whose thickness is 0.0000152 foot will thus be melted; so that if  $a$  be the mean distance of the earth from the sun expressed in feet, a spherical shell of ice, whose radius is  $a$  and thickness 0.0000152 foot, will be the volume of ice in cubic feet melted every second by the whole of the radiant solar heat. But one pound of ice has a volume equal to  $(0.2584)^3$  cubic foot, therefore  $\frac{4\pi a^2 \times 0.0000152}{(0.2584)^3}$  will be the weight of the ice in pounds thus melted.

Again, in order to melt one pound of ice, 79.25 thermal units are required; and thus the whole solar heat evolved in one second of time is equal to  $\frac{4\pi a^2 \times 79.25 \times 0.0000152}{(0.2584)^3}$  thermal units.

Now, by equating the heat generated to the heat evolved in one second, we get

$$\frac{4\pi z_0 r_0^3 \times 2431}{5 \times 1390} = \frac{4\pi a^2 \times 79.29 \times 0.0000152}{(0.2584)^3},$$

and the contraction  $z_0$  is therefore only 0.000004079 foot in one second, or 129 feet per annum; and, as we have already said, ages must elapse before the effect of this contraction can become visible to us, whether we compare direct measures of the solar disk or observed periods of axial rotation.

The contraction, therefore, is so small that as much allowance can be made for the assumptions introduced above as may be thought necessary, without altering our general conclusion in the slightest degree—namely, that the source of the solar heat and light is connected with the mechanical theory of heat by means of the contraction of the composing mass.

The application of this theory to other bodies is almost without limit; the earth has contracted, and has stored up a corresponding amount of heat in the non-conducting rocks and soils; the stars, by their intrinsic brilliancy, indicate the operation of the force of gravity upon contracting matter; the nebulae afford examples of the commencement of this operation; and periodical variations in light now become perturbations, the effect of disturbing masses in motion, producing endless changes subject to the great principle known as the conservation of energy.—*Monthly Notices of the Royal Astronomical Society*, April 12, 1872.

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A NEW SENSITIVE SINGING-FLAME. BY W. E. GEYER, OF THE STEVENS INSTITUTE OF TECHNOLOGY.

Philip Barry has recently described \* a very sensitive flame pro-

\* 'Nature,' vol. v. 30, Nov. 2, 1871. [This form of apparatus would seem

duced by placing a piece of ordinary wire gauze on the ring of a retort stand, about four inches above a Sugg's steatite pin-hole burner, and lighting the gas above the gauze. "The flame is a slender cone about four inches high, the upper portion giving a bright yellow light, the base being a non-luminous blue flame. At the least noise this flame roars, sinking down to the surface of the gauze, becoming at the same time almost invisible. It is very active in its responses; and being rather a noisy flame, its sympathy is apparent to the ear as well as to the eye."

A simple addition to this apparatus has given me a flame which, by slight regulation, may be made either:—(1) a sensitive flame merely, that is a flame which is depressed and rendered non-luminous by external noises, but which does not sing; (2) a continuously singing-flame, not disturbed by outward noises; (3) a sensitive flame, which only sounds while disturbed; or (4) a flame that sings continuously, except when agitated by external sounds. The last two results, so far as known to me, are novel.

To produce them it is only necessary to cover Barry's flame with a moderately large tube, resting it loosely on the gauze. A luminous flame six to eight inches long is thus obtained, which is very sensitive, especially to high and sharp sounds. If now the gauze and tube be raised, the flame gradually shortens and appears less luminous, until at last it becomes violently agitated, and sings with a loud uniform tone, which may be maintained for any length of time. Under these conditions, external sounds have no effect upon it. The sensitive musical flame is produced by lowering the gauze until the singing just ceases. It is in this position that the flame is most remarkable. At the slightest sharp sound it instantly sings, continuing to do so as long as the disturbing cause exists, but stopping at once with it. So quick are the responses, that by rapping the time of a tune, or whistling or playing it, provided the tones are high enough, the flame faithfully sounds at every note. By slightly raising or lowering the jet the flame can be made less or more sensitive, so that a hiss in any part of the room, the rattling of keys, even in the pocket, turning on the water at the hydrant, folding up a piece of paper, or even moving the hand over the table, will excite the sound. On pronouncing the word "sensitive" it sings twice; and in general it will interrupt the speaker at almost every "s" or other hissing sound.

The several parts of the apparatus need not be particularly refined. By the kindness of Pres. Morton I have used several sensitive jets of the ordinary kind made of brass; they all give excellent results. Glass tubes, however, drawn out until the internal diameter is between one sixteenth and one thirty-second of an inch, will do almost equally well. For producing merely the singing-flame, even the inner jet of a good Bunsen burner will answer. The kind of gauze too is not important: I have generally used a piece which

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not to be original with Mr. Barry, since identically the same thing, apparently, was described months earlier by Prof. Govi, of Turin, and noticed in the September Number of the '*Moniteur Scientifique*.'—Eds. of *Am. Journ.*]

has been rounded for heating flasks ; it contained about 28 meshes to the inch.

The tube chiefly determines the pitch of the note, shorter or longer ones producing, of course, higher or lower tones respectively. I have most frequently used either a glass tube twelve inches long and one and a quarter inch in diameter, or a brass one of the same dimensions. Out of several rough pieces of common gas-pipe no one failed to give a more or less agreeable sound. Among these gas-pipes was one as short as seven inches, with a diameter of one inch, while another was two feet long and one and a quarter inch in diameter. A third gas-pipe, fifteen inches long and three quarters of an inch in diameter, gave, when set for a continuous sound, quite a low and mellow tone. If the jet be moved slightly aside, so that the flame just grazes the side of the tube, a note somewhat lower than the fundamental one of the tube is produced. This sound is stopped by external noises, but it goes on again when left undisturbed. All these experiments can be made under the ordinary pressure of street gas, three fourths of an inch of water being sufficient.—Silliman's *American Journal* for May 1872.

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ON THE BEST RESISTANCE OF THE COILS OF ANY DIFFERENTIAL GALVANOMETER. BY LOUIS SCHWENDLER, ESQ.\*

Mr. Schwendler gave a short outline of his investigations, stating that it would be impossible for him to read the paper in full, on account of its intricate and purely mathematical character ; he would give, however, the general results obtained and show their advantages when applied, illustrating his explanations on the black board and by a differential galvanometer placed on the table. The paper itself would be published in Part II. of the Journal.

In that most common form of the differential galvanometer, when the two coils are fixed and of equal resistances and equal magnetic moments, Mr. Schwendler found that the following interesting and most simple relation should exist between the resistance of the galvanometer-coil and the resistance to be measured, in order to have the greatest possible sensibility—namely, that

*The resistance of the galvanometer-coil should be one third of the resistance under measurement*, supposing that the resistance of the testing-battery common to both the coils can be neglected against the resistance to be measured.

Mr. Schwendler remarked that the differential galvanometers at present employed in the Government Telegraph Department of India have a far too low resistance, and that this, to a certain extent, explained the great want of sensitiveness of these instruments when used for measuring resistances higher than 600 units.

If the coils had a resistance of about 2500 units each, instead of 200 only, the sensitiveness of these instruments would at once be nearly trebled—a fact which shows the great advantage to be derived from the application of Mr. Schwendler's results in the construction of differential galvanometers.

\* Abstract from the Proceedings of the Asiatic Society of Bengal, March 1872. Communicated by the Author.



THE  
LONDON, EDINBURGH, AND DUBLIN  
PHILOSOPHICAL MAGAZINE  
AND  
JOURNAL OF SCIENCE.

SUPPLEMENT TO VOL. XLIII. FOURTH SERIES.

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LXI. *On the Origin of the Earth's Magnetism, and the Magnetic Relations of the Heavenly Bodies.* By F. ZÖLLNER.

[With a Plate.]

[Concluded from p. 469.]

20.

IF variations in the solar radiation connected with the time of rotation could be shown to exist, this would, of course, have to be regarded as a support of my theory. As, however, heat does not exert a polar influence at a distance, and as, according to my theory, the poles of the currents on the sun's surface are the coldest parts, we ought to expect a double minimum of radiation caused by the nutation of the poles; and the magnitude of these minima would be influenced by the season. As heat does not possess any polar properties, it would be possible that the temperatures of the two hemispheres, and hence also their cold-poles, have different values.

I take the liberty to compare with these speculations the results of a comprehensive research by D'Arrest, now Director of the Observatory at Copenhagen, "*On the Unequal Distribution of Heat on the Sun*"\*. The name of the author is a sufficient guarantee of the correctness of his conclusions.

(Pp. 79-81.) "It has been proved lately, as is known, by thermoelectric experiments of Secchi in Rome, that the sun's heating-power is different for different heliographic latitudes. The decrease of heat from the sun's equator to the poles, which is undoubtedly indicated by the repeated observations communicated in the *Comptes Rendus*, recalls to our minds the inequalities in the heating-power, proved by Professor Nervander in Helsingfors in 1845, which show themselves in different heliographic longitudes. These inequalities make themselves percep-

\* Proceedings of the Royal Saxon Society of Sciences, July 2, 1853.

tible by the sun's rotation in the thermometric observations if they are combined for a sufficiently long period, so that the yearly and daily periods may be considered to be eliminated from the means. In the third volume of the *Bulletins* of the Ph.-math. Class of the Academy of Petersburg, Nervander has shown that the coefficient depending upon the rotation of the sun, as determined by the observations in Paris from 1816–1839, amounts to  $0^{\circ}302$  C. The difference in temperature, therefore, observed when the coldest and hottest meridians are opposed to the earth amounts to  $0^{\circ}604$  C. In the same paper this quantity is determined by the observations made in Innsbruck from 1777 to 1828; the number arrived at agrees perfectly with the results of Paris, and is  $0^{\circ}60$  C. The objection has been made that observations of two places only cannot prove the existence of inequalities directly depending on the rotation of the sun; but the probability of it would be considerably increased if, under very different climatic conditions, a periodicity of nearly the same magnitude should be deduced.

“In this respect, since 1845, only the Milanese observations at noon have been reduced by Carlini. With other observations, published in the mean time in Poggendorff's *Annalen*, such comparisons cannot be made as are required to decide first the real existence of the cause, before any consequence can be deduced from it. Carlini found the inequality arising from the rotation of the sun, according to the Milanese observations, to be  $0^{\circ}712$  C. The close agreement of this number with that found in Paris and Innsbruck is the more surprising, as the influence arising from inequalities in the sun's heating-power must make themselves a little less perceptible in higher geographical latitudes. The discussion of the Milanese observations comprises the years 1835 to 1844.

“In a similar manner I shall in this paper determine the coefficient arising from a possible influence of the sun's rotation from the observations made at the Observatory in Königsberg at noon from 1827–1837. Beginning with the meridian M (turned towards the earth on the 1st of January 1827 at noon, Königsberg time), I have put together in the following Tables in the first group the days and temperatures which belong to the same meridian M. The second group gives the days and temperatures belonging to the heliographic longitude  $M + 120^{\circ}$ , the third those of long.  $M + 240^{\circ}$ . The time of rotation of the sun is assumed to be  $27.26$  days”\*.

\* This period corresponding very nearly to the more reliable recent observations, was adopted by myself as well as by Nervander and Carlini, in order to make the results of the Königsberg observations more comparable with the results for Paris, Innsbruck, and Milan.

After having fully discussed the materials of observation, D'Arrest concludes his research with a comparison of the results obtained, in the following manner:—

(Pp. 99, 100.) "The sums for the different years are, in degrees Réaumur for Berlin at noon, as follows :

	1st group.	Obs.	2nd group.	Obs.	3rd group.	Obs.	4th group.	Obs.
1836-37.	118.1	14	127.7	14	82.0	13	87.2	13
1837-38	82.0	13	93.4	13	86.6	14	112.9	14
1838-39.	125.1	14	97.4	13	95.6	13	93.5	13
1839-40.	98.9	13	126.3	14	108.3	14	112.5	13
1840-41.	97.4	13	103.7	13	98.8	13	116.4	14
1841-42.	125.1	14	138.2	14	111.4	13	110.1	13
1842-43.	101.1	13	113.6	13	132.5	14	116.1	14
1843-44.	117.0	14	131.5	13	107.0	13	100.9	13
1844-45.	94.1	13	100.1	14	98.9	14	89.2	13
1845-46.	126.1	14	115.3	13	132.7	13	131.8	14
Sums ...	1085.1	135	1147.2	134	1053.8	134	1070.6	134

"We have therefore for the meridian M, which was turned towards the earth at the Berlin noon, July 1, 1836,

Mean temperature in degrees Centigrade	10.0475
For the meridian M + 90°	" 10.6989
" " M + 180°	" 9.8300
" " M + 270°	" 9.9863

A mere glance at these numbers shows that these observations do not lead to the coefficient determined above, but indicate a much greater variation in temperature arising from the sun's rotation. It is impossible to express by three constants the four values deduced from the Berlin observations, as this has been done for Königsberg; and we must therefore draw an interpolating line of about the following form :

$$\begin{aligned} \text{Mean temperature} &= 10.1406 + 0.3724 \sin (16^\circ 59' + m) \\ &\quad - 0.3615 \sin (33^\circ 57' + 2m). \end{aligned}$$

The degrees are Centigrade, and  $m$  runs through its values in 27.26 days. The curve has two maxima and two minima, found by the equation

$$\frac{\cos (16^\circ 59' + m)}{\cos 2(16^\circ 59' + m)} = 2.9377.$$

The solution gives

$$\cos (16^\circ 59' + m) = +0.12753 \mp 0.71850,$$

and hence for the



First minimum . .	10·0130 C. for $m = 15^{\circ} 14'$
Absolute maximum .	10·7857 „ 109 15
Absolute minimum .	9·4955 „ 246 47
Second maximum .	10·2682 „ 310 48

“It is remarkable that already Nervander (*Bulletins*, vol. iii. p. 14) suspected the existence of at least two minima and two maxima. If we must content ourselves to have proved the existence of a variation, and, as it seems, a variation without equal decrease and increase, while the coefficient of the inequality cannot be determined accurately from the observations at our disposal—still, if the sun belongs to the variable stars, the existence of two greatest and two smallest values corresponds to the phenomena of light observed on  $\beta$  Lyrae”\*.

It is evident that the hope to determine accurately the position of the poles of cold of the sun (which, according to my theory, must coincide with the magnetic poles) can much more easily be realized by actinometric than by thermometric observations of the temperature of the air. These observations can be made simply by comparing two thermometers put very near together, one of which has its bulb exposed to the sun for a certain number of seconds, whilst the bulb of the other is in the shade. The difference in the expansion of the mercury in the two bulbs furnishes us the means for a relative determination of the sun's radiation of heat. The height of the sun will, of course, have to be noticed and eliminated in a similar manner as in photometric observations. In this way we shall be able to prove in a comparatively short period the variations in the heating energy of the sun caused by the periodicity of the sun-spots.

## 21.

Partly from the recent observations of Secchi, and partly from not yet published communications of Professor Spörer in Anclam and Dr. Vogel in Bothkamp, it follows that all phenomena deduced theoretically by me find an unexpectedly speedy empirical confirmation. This regards as well the fact that the poles of the currents on the sun's surface do not coincide with the poles of

\* If Airy, in the ‘Astronomical Notices,’ No. 934, does not come to any decisive result as regards the variations of temperature caused by the sun's rotation, making use of the observations made at Greenwich during a period of only six years (1848–1853), this is partly explained by the different manner of reduction, and partly by climatic conditions. It is clear that the effect will be the less perceptible in the temperature of the air the more the air is filled with aqueous vapour and the sky covered with clouds and fog. The choice of D'Arrest, therefore (only to make use of the observations at noon, when the sun's radiation has the greatest effect), seems to me by far more rational than the mean daily temperature used by Airy.

rotation, as the real existence of currents in the sun's atmosphere, such as I have made use of to explain the velocity of rotation for different heliographic latitudes, in my paper "On the Law of Rotation of the Sun and the large Planets" (Feb. 11, 1871), and as I had already deduced them from general physical laws in my paper "On the Periodicity and Heliographic Distribution of Sun-spots" (Dec. 12, 1870). I will only mention here the results communicated by Father Secchi to the Paris Academy on July 24 and September 4, 1871.

Father Secchi says in his first communication\*:—"L'étude des protubérances nous a dévoilé des courants très-violents, qui dominant au-dessus de la chromosphère. J'ai fait une attention particulière à la direction de la courbure des jets les plus élevées, et j'ai trouvé que, en général, de l'équateur aux latitudes moyennes, la direction dominante est tournée vers les pôles."

In a second paper† Secchi mentions a great number of protuberances. He gives to those agreeing with the law of rotation the sign +, to those in opposition to it the sign -. He says at the end of his paper, referring also to the non-coincidence of the poles of rotation and those of the currents, as follows:—

"Je rappellerai que la loi supposée à vérifier était celle d'un entraînement général des protubérances élevées de l'équateur vers les pôles, de sorte que, dans les latitudes moyennes, nous devions rencontrer les inclinaisons dirigées vers les pôles: à l'équateur, on devait avoir une direction variable, et aux pôles une inclinaison nulle.

"Le résultat obtenu a été le suivant: pendant quarante-deux jours d'observations, on a obtenu,

+	protubérances conformes à la loi	. . . 403	} Rapport 2·92 : 1·00.
—	„ discordantes	. . . 138	
±	„ situées surtout près des pôles	102	

"Ces chiffres sont évidemment très-favorables à la loi hypothétique dont nous sommes partis; mais sa probabilité paraîtra encore plus remarquable après quelques réflexions.

"1°. Un grand nombre des discordances vient de ce que, comme on l'aperçoit clairement sur les figures, le grand tourbillon général (pour l'appeler ainsi) qui enveloppe le soleil n'est pas concentrique à l'axe géométrique de rotation, de sorte que le pôle de rotation reste tantôt à droite, tantôt à gauche du pôle de circulation. On reconnaît très-bien le pôle de circulation, car à ses extrémités les filets des protubérances sont ou presque verticaux ou absolument verticaux, tandis qu'à droite et à gauche

\* *Comptes Rendus*, vol. lxxiii. p. 242 et seqq.

† *Ibid.* p. 595 et seqq.

ils sont fortement inclinés. Nous n'avons pas tenu compte de cette particularité, et les exceptions paraissent plus nombreuses.

"2°. Une autre irrégularité provient de ce que l'activité solaire n'est pas actuellement la même dans les deux hémisphères, et il arrive que l'hémisphère le plus actif entraîne la circulation au delà de la limite équatoriale (comme il arrive chez nous pour les vents alizés), et il en résulte que l'équateur ne divise pas en deux régions égales les zones de circulation.

"3°. Enfin il faut tenir compte de l'influence des taches, qui troublent considérablement cette circulation."

These facts, derived by Secchi from numerous and careful observations, are of no importance to M. Faye; for, in a communication to the Paris Academy which I received whilst this paper was being printed, M. Faye says as follows:—

"C'est ainsi qu'on a cru récemment trouver une indication favorable à l'existence de ces courants dans les directions si variées des jets d'hydrogène incandescent émis par la chromosphère. . . .

"D'ailleurs la seule inspection des dessins déjà publiés en grand nombre suffit, *aux esprits non prévenus*, pour faire évanouir toute idée de courants généraux dans la chromosphère"\*.

In order to prove that the suspicion expressed against Father Secchi, that he was prejudiced in his observations, and belonged, as regards the currents in the solar atmosphere, to the "*esprits prévenus*," is without foundation, I take the liberty to state that I received in the month of April a letter from Father Secchi, dated Rome, April 28, 1871, in which he requests me to send him the above-mentioned paper, "On the Law of Rotation of the Sun and the large Planets," in which I had again given in detail the theoretical deduction of that law of circulation of the solar atmosphere the physical causes of which were first developed in my paper of December 12, 1870 †.

The words in question of Secchi's letter to me are as follows:—

"M. Schellen de Cologne me donne la nouvelle que vous avez fait un travail très-intéressant sur la rotation du soleil. Comme

\* *Comptes Rendus*, vol. lxxiii. p. 1128. M. Faye, in a discussion of my paper "On the Law of Rotation of the Sun and the large Planets," declares, "Suivant M. Zöllner, le soleil bien loin d'être à l'état gazeux, est entièrement solide, sauf une mince couche liquide, semblable à de la lave en fusion, qui le recouvre entièrement." I take the liberty to say that this view of the condition of the sun has *never* been expressed by me, and that the above declaration of M. Faye can only have resulted from a *superficial reading* of my paper. Evidently all arguments which M. Faye brings forth against my theory based on the above false view attributed to me fall to the ground.

† Proceedings of the Royal Saxon Society of Sciences, December 12, 1870.



tout ce qui vient de vous est très-intéressant pour moi, . . . je vous prie de me faire savoir où, et comment je pourrai me procurer cet intéressant travail."

I sent at once the desired paper to Rome. If, therefore, Father Secchi had undertaken his highly meritorious observations with the intention and desire to confirm the law of circulation derived theoretically by me, he would doubtless have mentioned my papers, which were known to him, and the necessary existence of those currents, which I had therein proved.

It is evident that through this circumstance the value and importance to my theory of the results found by Secchi is essentially increased, and that all "*esprits non prévenus*" must regard the probability of that theory, and of the causes from which it flows, as confirmed by *stringent* facts of observation.

If, on the large planets Jupiter and Saturn (as I have rendered probable for various reasons\*), the same causes for the circulation of their atmospheres and liquid constituents are in existence in consequence of the state of high temperature on their surface, the poles of these currents will also here not coincide with the poles of rotation. It is clear that then a strip parallel to the equator of currents, in the vicinity of its point of intersection with the equator of rotation, will, at the end of half a revolution, have undergone a change of direction amounting to double the angle of inclination of the two axes. Hence exact measurements of the angles of position of the strips on the large planets would show a period depending upon the duration of a rotation, and would therefore be appropriate for measuring the latter. Such researches will shortly be commenced in the observatory of Chamberlain von Bülow at Bothkamp.

It is clear that then the occurrences in the streams on the surface of the large planets must be connected by the same tie with the occurrences on the solar surface as the inner glowing streams of our planet are with the period of solar spots. It is to be expected, therefore, that we shall find also in the violent movements and manifold changing forms of the surface of Jupiter a period connected with the frequency of sun-spots.

22.

If we look at the variety of relations which have been deduced from the theory here developed of the physical causes of the earth's magnetism and have been confirmed by observation, we shall hardly be surprised that the influence of the sun-spot period on the inner temperature of the earth (which I have explained theoretically in § 15) has been recently ascertained by observation.

\* Proceedings of the Royal Saxon Society of Sciences, Feb. 11, 1871.

From the observations of temperature made at the Cape of Good Hope from 1841 to 1870 with very good instruments, Stone\* calculated the mean yearly values and represented them graphically by a curve. He compared with this a second curve indicating the frequency of sun-spots as given by the observations of Wolf, and found that, if he reversed the latter curve, it coincided so exactly with that of the mean yearly temperature, that he expresses the opinion that "the same cause which leads to an excess of mean annual temperature leads equally to a dissipation of solar spots."

In a letter to Sabine he says expressly: "I may mention that I had not the slightest expectation, on first laying down the curves, of any sensible agreement resulting, but that I now consider the agreement too close to be a matter of chance. I should, however, rather lean to the opinion that the connexion between the variation of mean temperature and the appearance of solar spots is indirect rather than direct, that each results from some general change of solar energy."

Piazzi Smyth had already made a communication to the Royal Society in April 7, 1870, in which he got the same result by other observations. He discussed and reduced the observations made in the years 1837 to 1869 with the four large earth-thermometers which had been sunk into the rocks of Calton Hill in Edinburgh by Principal Forbes, in consequence of a decision of the British Association. Amongst other periodical variations in the temperature of the earth, Piazzi Smyth found a period of 11.1 years, or the sun-spot period. Some circumstances discussed by Smyth in detail lead him also to the result that the sun-spots cannot be the true cause of the variation of temperature in the earth.

I think it very probable, after these results, that if thermometers could be sunk still deeper into the earth the influences would be much more decided, as the retarding influence of the incomplete conduction of the earth's crust would be less. It depends upon the degree in which these retarding influences may be annihilated whether the yearly period of magnetic disturbances and auroras will be found in these observations.

### 23.

Although in § 9 I have deduced theoretically the necessary connexion between mechanical and magnetical changes on the surface of the earth, we must yet inquire whether the frequency of mechanical reactions of the glowing liquid nucleus of the earth shows also periodical changes which coincide with the changes of those causes which can produce the mechanical changes.

\* Proceedings of the Royal Society, No. 127 (1871).

So far as these changes are generated by exterior causes, we have only to consider the sun and moon. It has been shown that these bodies may exert a double influence on the mechanical reaction of the liquid nucleus against the solid crust of the earth, viz. :—

1. A direct one, by generating a pressure- or tidal wave.

2. An indirect one, by magnetical induction.

The direct influence will appear most strongly with the moon, the indirect one with the sun.

The great incompleteness in which our instruments for exact measurement for sudden changes in the level of the ground are at present, reduces the mechanical reactions able to be observed to oscillations of the ground strong enough to make themselves perceptible to our senses in the form of earthquakes.

It is therefore to be asked whether we can find any connexion between the frequency of these phenomena and the path of the moon which would correspond to the influence discussed theoretically above. The influence of the moon on the moveable parts of the earth must be greatest when its distance from the earth is smallest or in the perigee, and then in the two positions in which the moon adds its attractive action to that of the sun or in the two syzygies. The minimum must fall on the two quadratures.

The most complete and detailed collection of communications regarding volcanoes and earthquakes is to be found in A. Perrey's *Bibliographie Seismique*\*. In a separate memoir† the author develops the relations of earthquakes and volcanic eruptions which are indicated by his collections, and advances numerous theorems regarding the nature of these phenomena. I take the liberty to add here the parts which are most important for our present purpose, in the words of the reporter of the Berlin Physical Society (1863), pp. 718 &c. :—

“The phenomenon of earthquakes is a complex one, for which we can with difficulty assume only one cause. Certain shocks, or series of shocks on a given place, lead to particular causes, of which a certain number may be independent of the general cause, and may have a modifying influence upon this.

“We observe a certain periodical return for which we recognize an influence of the moon; for in the month two maxima and two minima appear. The moon attracts the glowing liquid nucleus of the earth, and generates a pressure against its solid crust, which

\* *Mémoires de l'Académie de Dijon*, vol. iv. pp. 1-112, vol. v. pp. 183-253, vol. ix. pp. 87-192, and vol. x. pp. 1-53. The first register appeared in 1855, the latter in 1863.

† “Propositions sur les tremblements de terre et les volcans, adressées à M. Lamé, Paris, 1863,” *Silliman's Journal* (2) vol. xxxvii. pp. 1-10.



may be different according to its strength and direction, and according to the condition of the inner surface of the solid crust. All phenomena appear here which may be produced by the propagation of waves with unequal velocities &c.

"The author concludes with remarks and calculations on the frequency of earthquakes during the latter half of the eighteenth century, in relation to the position of the moon, and on the frequency of the phenomenon with regard to the passage of the moon through the meridian. The greater frequency at the time of the syzygies in contradistinction to the quadratures, and in the perigee in contradistinction to the apogee, is confirmed."

The indirect influence of the sun would be shown by the influence of the sun-spots on the frequency of the earthquakes. I do not know of any fact to that effect.

I take the liberty to communicate a passage from a memoir by Mallet\* (in the words of the reporter of the *Berliner Berichten*, 1863, p. 926), respecting the undulatory character of the earthquake observed in England during the night of October 5-6, 1863.

"With respect to the explanation of the conception, Mallet expresses himself decidedly against the opinion that the earthquake is a means to produce lasting geological upheavings. He thinks it is rather the passage of a wave, or of several waves of elastic compression, in any direction, from the vertical to the horizontal, in every azimuth, through the mass and surface of the earth, commencing from one or more centres of percussion. Tones and surging motions can be traced, which depend on the given percussion and on the distribution of land and water.

"Mallet then gives a short abstract of the history of earthquakes. The right view of them has only been obtained since the brothers Weber, and Scott Russell, developed the laws of certain undulatory motions, and, chiefly, the latter the laws of the motion of translation. He then considers the phenomena of earthquakes up to a certain point."

It is to be seen from these data that the movements of the ground may be perhaps continuous, but then only so weak that they cannot be observed without more delicate aids. At any rate we may assert that the frequency of positively ascertained movements of the ground would be considerably increased if we possessed delicate seismometrical instruments.

#### 24.

Two years ago I had shown and explained to the Royal Society

\* "The late Earthquake and Earthquakes in general," *Quarterly Journal of Science*, vol. i. pp. 53-69 (1863).

an instrument\*, the principles of which, as I learned afterwards, had already been proposed seven years previously by Perrot to the French Academy for the same purpose†.

I have not heard any thing about the actual employment and exact carrying-out of the principle until now. I therefore take the liberty to describe the instrument, which has proved good in numerous experiments, and hitherto the most convenient for the measurements made by it. In order to explain the purpose of the apparatus and its relations to the present question, I take the liberty to reproduce a passage from my paper cited below\*.

“The methods hitherto employed for measurements of attractive and repulsive forces may be divided into two classes:—the first one, in which the forces act on masses allowed to turn round a horizontal axis, as in the case of the pendulum, different electrometers, and similar apparatus; and a second one, in which the masses may turn round a vertical axis, as, for instance, in the different torsion-balances. The apparatus of the first class have one-armed levers; those of the second class have two arms, and are therefore only applicable for forces which are not parallel. The instruments of the first class are not liable to this restriction, but can only be employed if the forces to be measured do not differ much from the force of gravity, as this is the force set against them. The instruments of the second kind may be employed for very weak and not parallel forces, as the momentum of direction may be diminished indefinitely.

“If we could devise a method which would possess the advantages of both of these classes, it would become of great importance in astronomy, as we should be enabled by it to measure even those small forces which are, for instance, produced by the difference in distance between any point on the earth’s surface from sun or moon and the distance of the earth’s centre of gravity, or by difference in the centrifugal force of two points at different distances from the earth’s surface.”

I explained the principles of such a method and its practicability in an apparatus for which I proposed the name of

“Horizontal Pendulum,”

in order to distinguish it from other pendulum-like instruments, also suspended by two threads. I gave the description of this pendulum with some remarks about its sensitiveness in the following words:—

“The end of a thin rod of glass 210 millims. long is joined to the end of a fine steel wire of 170 millims. length; the other

\* Proceedings of the Royal Saxon Society of Sciences, November 27, 1869, “On a new Method for the Measurements of Attractive and Repulsive Forces.”

† *Comptes Rendus*, vol. liv. p. 728.

end of this wire is fixed to a projection 20 millims. long at the foot of a vertical brass stand. The end of a second wire, of equal length, is attached to the glass rod at 10 millims. distance from the point of attachment of the former, and is fixed to a projection at the upper end of the stand, nearly vertically over the point of attachment of the first wire.

"A micrometer-screw of the stand enables us to diminish indefinitely the momentum of direction of the 'horizontal pendulum.' It is reduced to zero if the two points of attachment lie in the same normal, supposing we neglect the torsion of the wires. Having regard to this torsion, however, the two points must lie in a line which is a little inclined to the normal.

"The described horizontal pendulum was put up in the cellar of the university buildings, which is 12 feet deep; the temperature in it is very constant. A mirror attached to the end of this pendulum allows us to read off the changes in the direction according to the method of mirror-readings on a scale 2500 millims. distant from the mirror. It is easily conceived that this apparatus can indicate very small changes of the normal with respect to the horizon. When the time of vibration was brought to 52 seconds by means of the tangent-screws, a deviation of 10 millims. on the scale corresponded to an inclination of the horizon amounting to 0.035 second of an arc; and as the tenth part of a division may be estimated with surety, 0.00035 second of an arc could be perceived. One can easily form an idea of the sensitiveness of the apparatus if I remark that the difference in pressure which was caused in the foundations of the building when a lecture-room on the second floor was filled, originated a deviation in the dot of light of twenty divisions of the scale. When the lecture-room was empty again, the mirror regained its former position. This instrument is therefore at the same time an exceedingly sensitive seismometer.

"The apparatus are, of course, protected by suitable covers from the currents of air and influences of radiant heat."

After many trials and much trouble\* I constructed the ap-

\* I discovered in the above-mentioned cellar a space lying about 15 feet deeper, which led directly to the bottom of the foundations. It was, however, too narrow to receive the telescope and scale down; so that I was obliged to content myself with bringing only the instrument down and reflecting the image of the scale into a telescope directed vertically downwards. When the whole instrument was protected by a brass cover, which had only an opening closed by a plane glass plate opposite the mirror, the opening leading to this subterranean space was shut up, with the exception of the parts necessary for the reading-off. The instrument was thus excellently secured from disturbing changes in temperature. But the above-mentioned elastic compression of the building, or rather the variation of level in the fundamental plane of the building, could still be observed; the magnitude, however, of the deviation was reduced to about two thirds of its original value.



paratus, represented in Plate III. which accompanies this paper not quite one sixth of its natural size. In place of the thin wires, on which the known changes in the position of equilibrium acted disturbingly, I used thin watchsprings,  $a, a'$ , which were held in continual tension by the weight  $A$  with the mirror  $c$  in front. I preferred to give to the instrument the greatest possible dimensions and weight, as the influences of sudden changes in temperature and the currents of air accompanying those changes were diminished accordingly. The stand is made of iron; and the feet of the tripod are as long as possible, in order to effect very small changes in the position of the points of suspension with regard to the direction of gravitation by the slow movement of the screws.

The screw  $d$ , which is situated as nearly as possible in the vertical plane passing through the two points of suspension  $c$  and  $c'$ , allows us to change as required the sensitiveness of the instrument, as by the relative position of the points  $c$  and  $c'$  the time of vibration of the horizontal pendulum is determined. A time of vibration of 30 seconds (half a period) is easily accomplished.  $B$  is a counterpoise to  $A$ . Before the oscillating mass  $A$  and the parts belonging to it were placed in the rings, which fit into small incisions cut into the cylindrical axis, it was set into vibration by the direct action of gravity round a knife-edge occupying provisionally the place of the turning-point. The time of oscillation amounted to nearly 0.25 second. By means of a known relation, the ratio of the moments of direction are thus easily obtained which are exerted by gravity on the vibrating mass in the horizontal and vertical directions.

The theory of the instrument for very small elongations, which alone need be here considered, is very simple; so that I shall content myself with communicating a few values obtained from the numerous series of observations which have been made during the years 1870 and 1871, in order to enable the reader to judge of the applicability and sensitiveness of the instrument.

I must, however, first make some remarks on the setting-up of the instrument. As the disturbances in the centre of the town were too frequent, it was transported into a little dome built in the garden of the observatory for astrophysical researches. Close to it I had a massive pillar of sandstone erected, covered in its whole extent by an insulated case, which, similarly to all refrigerators, was provided with thick walls filled with bad conductors of heat; on the inner side the walls were covered with tin. The case was surrounded by a small wooden structure, with an interval of about half a foot, allowing the air to circulate freely, but protecting the first box from any direct influence of radiant solar heat. On one side of the building doors were in-

troduced, so as to allow free access to the instrument. The observations were made from the inner space of the small observatory, one wall of which was broken through for this purpose; through this opening the mirror of the pendulum, shut up in a separate box with a plane glass plate, was observed.

I now take the liberty to communicate an observation extended over several hours, which I made on September 18, 1870, during the evening hours from 6<sup>h</sup> 35<sup>m</sup> to 10<sup>h</sup> 35<sup>m</sup>.

*Observation with the Horizontal Pendulum.*

Distance of scale from mirror = 3186 millims.

Duration of a vibration . = 14.444 seconds.

With regard to the above-mentioned time of vibration of 0.25 second, we calculate that a millimetre-division on the scale corresponds to a deviation of 0.0097063 second of an arc of an ordinary pendulum. The numbers given in the Table indicate the readings of the values of the elongations.

Leipzig, September 18, 1870.

No.	Time.	Reading.	No.	Time.	Reading.	No.	Time.	Reading.
1.	<sup>h</sup> <sup>m</sup> 6 35	80.0 } 76.8 } 78.4	15.	<sup>h</sup> <sup>m</sup> .....	78.9 } 80.9 } 79.9	29.	<sup>h</sup> <sup>m</sup> 7 52	79.8 } 82.4 } 81.1
2.	.....	79.7 } 77.0 } 78.3	16.	7 35	79.0 } 81.6 } 80.3	30.	.....	80.0 } 82.2 } 81.1
3.	.....	80.0 } 77.2 } 78.6	17.	.....	79.4 } 82.0 } 80.7	31.	.....	80.0 } 82.4 } 81.2
4.	.....	80.4 } 78.0 } 79.2	18.	.....	80.0 } 82.2 } 81.1	32.	8 0	80.7 } 82.2 } 81.9
5.	.....	80.1 } 77.9 } 79.0	19.	.....	80.3 } 82.3 } 81.3	33.	.....	80.9 } 82.0 } 81.9
6.	.....	79.6 } 77.8 } 78.7	20.	.....	80.0 } 82.0 } 81.0	34.	8 2	80.8 } 82.0 } 81.9
7.	.....	79.5 } 78.3 } 78.9	21.	7 35	80.5 } 82.0 } 81.2	35.	.....	80.8 } 82.0 } 81.9
8.	.....	78.1 } 80.1 } 79.1	22.	.....	80.0 } 81.6 } 80.8	36.	.....	80.5 } 81.6 } 81.0
9.	.....	78.2 } 80.3 } 79.2	23.	.....	80.3 } 81.7 } 81.0	37.	.....	80.4 } 81.8 } 81.1
10.	.....	78.8 } 80.5 } 79.6	24.	7 38	80.0 } 81.6 } 80.8	38.	8 4	80.9 } 82.0 } 81.7
11.	.....	78.7 } 80.6 } 79.7	25.	7 50	79.3 } 82.3 } 80.8	39.	10 0	81.5 } 85.0 } 83.2
12.	6 53	79.0 } 80.5 } 79.7	26.	.....	79.4 } 82.7 } 81.0	40.	.....	81.0 } 85.0 } 83.0
13.	.....	78.8 } 80.4 } 79.6	27.	.....	79.3 } 82.8 } 81.0	41.	10 35	83.9 } 86.4 } 85.1
14.	.....	78.9 } 80.3 } 79.6	28.	.....	79.6 } 82.4 } 81.0	42.	.....	83.9 } 86.3 } 85.1

These numbers will be sufficient to prove the great sensitive-

ness of the instrument, and the surety with which we can observe deviations of only 0·001 second of arc, even with a time of vibration of 14·44 seconds. The above would be the value corresponding, according to the above relation between one division of the scale of the horizontal and one of the vertical pendulum, to about 0·1 division of the former.

We can easily increase to four or five times the sensitiveness of the instrument by raising the foot with the adjusting-screw *d*; but for this the stability of the pillar and its surrounding parts was not sufficient, in spite of all precautions. A railway-train passing at a distance of about  $1\frac{1}{2}$  kilometre caused undulatory motions of the ground, which called forth periodic variations of the position of equilibrium of the horizontal pendulum.

Such observations are therefore best made in deep and quiet mines, where the variations of temperature and shakings of the earth are absent, and all those influences can therefore be determined quantitatively which are caused either directly by volcanic movements of the ground, or indirectly by magnetical induction through changes in the velocity of the streaming masses on the inner part of the earth.

Besides the generation of an inner tidal and pressure-wave, the sun and moon exert a direct influence on the position of the instrument, because they attract with different intensity the centre of gravity of the earth and the centre of gravity of the pendulum. The magnitude of the deviation of the plumbline has been made the subject of an interesting research by C. A. F. Peters\*.

The mean deviation which the moon can generate in its most favourable position amounts, according to the deductions of Peters, to 0''·0174; that which the sun generates under the same conditions amounts to 0''·0080.

If the horizontal pendulum is put up, as mine is, so that the position of equilibrium lies in the plane of the meridian, it is clear that the above maximum values of deviation must have opposite signs, according as the heavenly body is to the east or to the west of the meridian; the angular difference will therefore be doubled in both cases—that is,

For the lunar influence = 0''·0348,

For the solar influence = 0''·0160.

The inaccuracy of the observations given above would therefore, after one reading, amount to  $\frac{1}{35}$  of the amount to be measured in

\* "On the small Deviations of the Plumbline and Level which are caused by the Attraction of the Sun, the Moon, and some Terrestrial Bodies. By Dr. C. A. F. Peters," *Bulletin de la Classe physico-mathématique de l'Acad. Imp. des Sciences de St. Pétersbourg*, tome iii. No. 14. Petersburg, 1844.



the case of the moon, and  $\frac{1}{16}$  in that of the sun. But, as already stated, the exterior conditions under which the observations have been made are very unfavourable for the purpose in question; so that in mines, under the supposition that the reactions of the glowing liquid nucleus of the earth are not magnitudes of the same order, the sensitiveness of the instrument would be considerably increased. We thus may even expect to determine the magnitudes on which the just-mentioned influences depend by a much extended and statistically treated series of observations,—that is to say, the masses and distances of sun and moon in units of the mass and radius of the earth.

Another subject of interest is connected theoretically with the observations of the horizontal pendulum. Supposing the instrument is set up as mentioned in the meridian, the pendulum, if moving only under the influence of the sun, would pass in twenty-four hours four times through its position of equilibrium in the meridian—at sunrise, at sunset, and at the upper and under passage of the sun through the meridian. As the movement of the pendulum is not an effect of summation as that of the sea in the tides, but is generated directly by attractive action at a distance, it must take place simultaneously with the corresponding true position of the sun. But if gravity, as light, takes a time of about eight minutes in arriving from the sun to the earth, the above positions of equilibrium would take place so much later. If, therefore, we only succeed in determining these positions to within one minute of accuracy, the question whether gravity needs time for its propagation could be decided, even if this velocity were ten times that of light.

The future, and persevering observation, must decide whether the rigidity of the crust of our earth\* is sufficiently great, in proportion to the above-mentioned influences, to allow such observations to be made with success. At any rate, systematical ob-

\* Several attempts have been made, chiefly by Hopkins, Sir W. Thomson, and Pratt, to decide the question whether the nucleus of the earth is solid or liquid, whether its crust is thick or thin, moveable or immovable. The researches relating to this question have, on the one hand, taken into consideration the influences of these properties on the constant of precession, and, on the other, transferred directly the observations made on the rigidity of solid bodies to the conditions of cohesion of the earth. It follows directly from the phenomena of the tides, that the crust of the earth is not to the fluid nucleus as a swimming island or a covering of ice to the sea. But to determine the degree of rigidity of the crust of the earth (which is compressed by immense forces according to the principles of cellar-vaults) from the forces which the moon exerts on these masses is, I think, just as hazardous as to draw a conclusion on the inner condition of an egg from its deformation by outward forces. As long as the forces are not strong enough to break the egg, we shall hardly ascertain in this way whether the egg is raw or boiled.

servations made simultaneously in two vertical circles with the horizontal pendulum, and their comparison with the readings of magnetical instruments, will supply valuable material. Their discussion will elucidate the causes of the close relation between the mechanical, electrical, and magnetical phenomena on our planet. We come by this into possession of an exceedingly rich language of signs, the explanation of which will perhaps some day give us as clear a conception of the occurrences in the earth as the language of signs of the senses, chiefly by the help of light, has given us of the occurrences on its surface.

### *Appendix.*

In conclusion of the present research on the Origin of the Earth's Magnetism and the Magnetical Relations of the Heavenly Bodies, I return to those considerations which formed the starting-point of my views of the physical causes of the electric currents which circulate round the earth from east to west, and thus call forth the magnetical and electrical phenomena on the surface of the earth. The reasons developed there, by which I was inclined to regard the connexion between electrical currents and streaming motion of liquids as a fundamental fact, grew, in the course of the whole research, into such a conviction that I cherished the desire to show the existence of these currents directly in flowing water.

Before going to the description of the simple experiments which seem to prove indeed the existence of such currents of very considerable electromotive force in streaming water, always in the same direction as the current of water, I may mention here those observations which have recently proved the constant existence of electrical currents in the crust of the earth. The literature on these earth-currents is already voluminous; its beginning is chiefly from the year 1859, when strong earth-currents rendered the correspondence by telegraph on all lines impossible on the occasion of an aurora taking place on August 29 with rare splendour, which I myself observed in Bâsle until morning-dawn. Through the acquaintance of a telegraphist I had opportunity to convince myself on the following morning at 7 o'clock of the force with which the armature of the electro-magnet still remained permanently in contact with the ends of the horseshoe.

In the year 1859 Lamont\* had stretched telegraph wires in the observatory of Munich from east to west and south to north and joined them to end-plates. The *electric current which was constantly flowing* was during a long time observed and registered hourly in the observatory by means of galvanometers. It

\* The Earth-current and its Connexion with the Magnetism of the Earth. Leipzig, 1862, pp. 1-74.

was found that *small movements* in the horizontal intensity of the terrestrial magnetism coincided with simultaneous *small changes* of the current in the line running from east to west.

When magnetic instruments and galvanometers of greater sensitiveness were employed, *a complete analogy was found to exist between the current in the wire running from east to west and the intensity, and then between the current of the wire running from north to south and the dip.* Moreover it was found, by comparison of the movements in the wires which were stretched along the astronomical and magnetical meridians, that the movement of the earth-current takes place chiefly parallel with the equator.

Already, before these observations of Lamont's, Walker\* had occupied himself with similar researches, which, however, chiefly referred to the great and extraordinary movements of the earth-current, and were therefore made on the telegraph-lines. He found that the earth-current was directed from north-east to south-west, or *vice versâ*, and in a line inclined at an angle of  $41\frac{3}{4}^{\circ}$  to the astronomical meridian.

It is remarkable, however, that in telegraph-lines lying in the same direction the earth-current is not always equally intense; some lines are even found in which the earth-current never shows great intensity.

Very extensive researches on the earth-current have been made by Matteucci† and Secchi‡.

Matteucci laid two wires covered with gutta percha, each 6 kilometres long, one in the magnetic meridian, the other in a direction at right angles to it. At the ends ditches of 2 metres depth were sunk. In the middle of each ditch a hole of  $\frac{1}{3}$  cubic metre was made, the sides of which were covered with clay, and filled with water. Porous vessels filled with a saturated solution of sulphate of zinc were then placed in these holes, in which well-amalgamated zinc plates were laid which were in communication with the wires. In this manner the plates were

\* "On Magnetic Storms and Earth-currents." Proceedings of the Royal Society, vol. xi. pp. 105-111; Phil. Trans. 1861, pp. 89-131. Other Literature in 1861:—

H. Lloyd, "On Earth-currents and their Connexion with the Phenomena of Terrestrial Magnetism," Phil. Mag. Ser. 4. vol. xxii.

B. Stewart, "On the great Magnetic Disturbance of August 28 to September 7, 1859, as recorded by Photography at the Kew Observatory," Proceedings of the Royal Society, vol. xi.

Airy, "On Spontaneous Terrestrial Currents," Reports of Brit. Assoc. 1861 and 1862.

† "Sur les Courants Electriques de la Terre," *Comptes Rendus*, tome lviii. p. 942 *et seqq.* (1864).

‡ "Sur les Courants de la Terre et leur Relation avec les Phénomènes Electriques et Magnétiques," *Comptes Rendus*, tome lviii. p. 1181 *et seqq.*



secured from all chemical influence; and as precautions were taken to keep the amalgamated plates at the same temperature, Matteucci expresses his conviction that there could not have been any thermoelectric currents. The observations made during one month gave the following results:—

1. Currents were almost always present.
2. Deeper ditches and wet weather gave an increase of the currents.
3. With the original depth of the ditches an increase in the dimensions of the porous vessel or the zinc plates did not cause a stronger current.
4. In the line running from north to south the direction of the current was always constant, and was flowing in the stretched wire from south to north. The intensity of this current showed a daily period with two maxima and two minima: the minima take place at noon and midnight (an hour sooner or later); the maxima take place at 5–7<sup>h</sup> in the morning, and 3–7<sup>h</sup> in the afternoon.
5. The line running from east to west shows great inconstancy in the intensity of the current (according to the galvanometer from 0° to  $\pm 15^\circ$ ) as well as in its direction; the direction from west to east in the wire, however, is the most common.
6. The connexion of the northern or southern plate with the eastern or western gave generally a current which was equal to the current in the south-north line.
7. The direction and intensity of the north-south current was independent of the temperature of the air, which varied between 0° and +18°; it was independent of the humidity of the air; neither storms nor thunderstorms had any influence.
8. Nothing was changed in the results when the wires which were covered with gutta percha were taken off the telegraph-posts and laid upon the earth.

What, asks now Matteucci, is the cause of these currents? He declares that he is not able to give any decisive answer. But he is inclined to assume a *close connexion with the terrestrial magnetism*.

A not less interesting fact of observation is communicated by Matteucci in a second paper\*. A constant and very remarkable connexion between the direction of the current and the difference in level of the two zinc plates. *Every wire which is connected with the ground at points of which one is deeper than the other, shows a pretty constant current which goes in the wire from the deeper to the higher station, and which at every atmospheric discharge receives a sudden increase, which, however, only lasts for an instant.*

The experiments were made on four different lines, of a length

\* *Comptes Rendus*, tome lix. p. 511–516.

varying from 600 to 36,000 metres, and a difference in depth of 83 to 642 metres. They always gave concordant results ; but *the longer wire and larger difference in height gave a stronger current.*

As regards the quality of these currents, Matteucci discusses expressly the question whether they are to be regarded as branch currents. He answers the question in the negative, as the conductivity of the earth must be assumed to be infinitely great in comparison with that of the wires.

According to my opinion, nothing else remains than to consider the layer of earth between the zinc plates electrically active, similarly to a voltaic pile. The fact *that the intensity of the observed currents increased with the interposed layer of earth* would be in harmony with this assumption.

The direction of the current within the earth would, of course, be the opposite of that in the wires, so that the surface of the earth would be surrounded by currents flowing from east to west and from north to south.

With regard to the diaphragmic currents of Quincke, it would be interesting to see whether the moisture constantly descending in the earth under great pressure would not be sufficient to generate these currents. Such a movement of the moisture in the porous layers of earth would also take place from the colder and damp regions to the hotter and dry regions, and give rise to electric currents directed in the same direction. The free tension of the thus excited electricity could, if sufficiently strong, make itself perceptible in the electricity of the air.

In this respect Secchi's above-mentioned researches offer still more complete material for the decision of the question here stated.

Two telegraph-lines were constructed, one running from north to south, the other directed from east to west. On both hourly observations were made from 6 o'clock in the morning to midnight, from June 1 to 16, 1863.

The following are the results of these observations :—

1. The line running from east to west shows much stronger variations than the north-south line.

2. The maxima of one line are coincident with the minima of the other ; this is true for the secondary as well as for the primary turning-points. The chief turning-points are at 7–8<sup>h</sup> A.M. (minimum of north-south and maximum of the east-west line) and 11–12<sup>h</sup> at noon (maximum of north-south and minimum of the east-west line).

3. During the night the currents are stronger and remain pretty constant.

Father Secchi draws from these experiments the conclusion that it is sufficient to observe only one line, and limits his further

observations to the north-south line, for which the position of the galvanometer was recorded ten times daily during a whole year.

The monthly means of this series of observations were then compared with the simultaneous readings of the bifilar instrument, and with those of the atmospheric electricity. *The result was, that the turning-points of all three classes of phenomena corresponded to each other.*

These facts of observation will be sufficient to show their close connexion, first, with my physical theory of the origin of the earth's magnetism, and, secondly, with the experiments to be described presently.

I took a galvanometer made by Sauerwald for the observation of thermo-currents with a reflecting-mirror, and inserted the ends of the copper wire into a gutta-percha tube, into which I introduced a current of water from the water-pipes. The water, after having passed through the tube, was made to flow into a vessel partly filled with water and *not insulated*. The galvanometer always showed a deflection of several degrees, indicating a current in the direction of the flowing water.

*The wider apart from each other I immersed the ends of the copper wire in the water, the greater was the deflection, so that the whole streaming mass must have acted in all its parts similarly to a voltaic battery if the observed deflection was not due to a secondary current.*

After my expectations had been fulfilled in this manner by the above rough experiments, I changed the mode of experimentation.

The following is a description of such an experiment, with numerical results.

I used a glass tube 500 millims. long and 8 millims. in diameter, with two holes in its sides at a distance of 380 millims. from each other. The holes were closed air-tight by a cork, by means of which two thin copper plates were introduced into the tube in such a way that they were washed by the flowing water. The two ends of the glass tube were joined to gutta-percha tubes of about its own length, either of which could be attached to the water-pipes, so that the current of the water could be reversed.

The following are the readings of the galvanometer in three experiments:—

Water not flowing.	Water flowing.	Difference.
369.9	362.0	— 7.9
370.0	362.0	— 8.0
371.0	363.0	— 8.0

Distance of the galvanometer from the scale 2500 millims.

In a second experiment the copper plates were not at all in-



roduced into the water. They were placed in the sides of two T-shaped glass tubes, so that the flowing water could not touch them. The two T-shaped tubes were united by a gutta-percha tube 1220 millims. long, so that the length of the column of water was now about 3·2 times as great as in the first experiments. The following Table shows the results:—

Water not flowing.	Water flowing.	Difference.
399·0	383·0	— 16·0
399·0	383·0	— 16·0

Current of water reversed:—

Water not flowing.	Water flowing.	Difference.
398·5	413·0	+ 14·5
398·5	412·5	+ 14·0

The direction of the electric current was always the same as that of the current of water.

In order to attain an approximate value for the electromotive force, I introduced into the quiet water a current of one of Grove's cells; I observed a deflection of 60°. Hence the electromotive force produced by the flow of a current of water 1220 millims. long was about the fourth part of that of a Grove's cell. I suspect that the cause of these currents is the same as that of the currents observed by Quincke. If, therefore, Quincke in his first paper, "On a new Class of Electrical Currents," says (p. 13), "When the tubes were fixed together without diaphragm, no deflection of the galvanic needle was observed; we see, therefore, that the diaphragm is necessary for the production of the electric current," the cause of the failure is probably that the column of water was too small, and most likely insulated.

I believe that Adie\* in the above-mentioned experiment (p. 350) has observed those electrical currents which, as I hope to prove by the above and other experiments, are necessarily connected with every streaming motion; for the moving forces, acting upon the elements of a flowing mass of liquid, are not in equilibrium as long as the movement takes place, and I am inclined to look at the partial difference in pressure existing in the direction of the currents as the cause of the electrical currents. Experience shows that wherever such differences are called forth in bodies, they may become the source of electricity by adequate combination and under suitable conditions†.

\* Adie tries to explain the currents observed by him by the influence of the oxygen of the atmosphere, according to the analogy of gas-batteries. He says in the paper cited, p. 383, "With both plates in still water and a tube filled with oxygen inverted over one, the effect was the same."

† The manuscript of the Appendix was delivered to the printers on February 13, 1872.

LXII. Reply to "*An Examination of the recent attack on the Atomic Theory.*" By C. R. A. WRIGHT, D.Sc. Lond.\*

WHEN there is a difference of opinion about matters of fact, it is usually traceable to the use of the same words in different senses; wherefore it is always advisable to preface a discussion by strictly defining the particular meanings attached to the words employed. Mr. R. W. Atkinson refers to my paper "On the Relations between the Atomic Hypothesis and the Condensed Symbolic Expressions of Chemical Facts and Changes (Dissected Formulæ)" as an attack on the *Atomic Theory*; but the terms atomic hypothesis (as I employ it) and atomic theory (as the latter is employed in University College Laboratory) mean two utterly different things—so much so, that Dr. Williamson, in his admirable essay "On the Atomic Theory" (Chem. Soc. Journ. 1869, p. 328), denies that notions which I have understood as necessarily involved in the atomic hypothesis are essential to the atomic theory. For instance, he exemplifies the meaning of the term "atom" thus (p. 364):—"In potassic hydrate oxygen is combined with hydrogen; it is also combined with potassium; but the hydrogen cannot pass off even at a red heat in combination with its half of the oxygen. The two halves are inseparable; and when I say that in a molecule of potassic hydrate there is a single atom of oxygen, I merely express that fact;" in other words, Dr. Williamson uses the term atom in this instance to express only *facts* which are otherwise expressible thus: "the radical O is bivalent in reaction where the substance designated by the formula KOH is concerned; and this substance, unlike the analogous thallos hydrate, TlOH, does *not* undergo a reaction such as that indicated by the symbols  $2\text{KOH} = \text{HOH} + \text{KOK}$  by the action of heat." Later on, Dr. Williamson refers to atoms thus (p. 365):—"The question as to whether our elementary atoms are in their nature indivisible, or whether they are built up of smaller particles, is one upon which I, as a chemist, have no hold whatever; and I may say that in chemistry the question is not raised by any evidence whatever. They may be vortices such as Thomson has spoken of; they may be little hard indivisible particles of regular or irregular form." And again (Discussion, p. 434), "Whether the smallest particles of matter have a spherical form or not, whether they are in their nature indivisible, whether they are in reality the ultimate atoms of matter, or like the planets of this system, he knew not, nor did such questions exist for him as a chemist. He therefore thought it wise to exclude them, important as they were, from the existing atomic theory."

\* Communicated by the Author.

From these quotations, together with the statement (p. 339) that "the so-called law of multiple proportions has no existence apart from the atomic theory," and the phrase (pp. 334 and 336) "theory of multiple proportions," it is evident that what Dr. Williamson accepts as the meaning of the term atomic theory is not the same as that which I have attributed to the phrase atomic hypothesis; which latter, as accepted by Dalton and Berzelius, may be thus enunciated, the terms Generalization, Convention, and Hypothesis being previously defined. A *generalization* is a statement whereby a large number of observations or experimental facts are expressed with either absolute or approximate accuracy; thus the statements that mammalia are vertebrated, that nitrates are soluble in water, that gases expand  $\frac{1}{273}$  part of their bulk at  $0^{\circ}$  for each  $1^{\circ}$  C. increase of temperature, are generalizations: in the first case there is absolute accuracy; for no mammalia which are not vertebrated are known; in the second and third there is approximate accuracy; for extremely few nitrates are known that are not readily soluble in water; and in the third case, which is perhaps the most to the point in the present discussion, no gas is known the behaviour of which is rigorously in accordance with the statement at all temperatures; but no gas is known which does not, under certain defined conditions, exhibit a behaviour which is in close proximity to that stated to occur.

The term *convention* is applied to an expressed or tacit understanding that a given mark, symbol, or name, &c. shall be applied to a given proposition, fact, object, &c. Thus the statement that the letter A represents in England a particular sound, that the sign  $x^2$  represents unity taken  $x$  times, and the resulting sum or product taken  $x$  times again, are referred to as conventions.

The term *Hypothesis* is applied to some proposition incapable of direct proof (at least in our present state of knowledge), but by admitting which a number of facts may be coordinated and shown to be correlated, and by means of which one can inductively argue from the known to the unknown.

Now the two terms *Theory* and *Law* appear to be employed by different writers in different acceptations. The majority refer to a theory as being simply an hypothesis in a further state of development: when the predictions deducible from an hypothesis are found to be mostly verified by experiment and observation, the term theory is applied to it; and when no exceptional case is known with which the theory cannot grapple and which it does not elucidate, the term *Law of nature* is then employed; so that Hypothesis, Theory, and Law are three terms of a similar nature, differing only in their range of application. Others,



nowever, employ the term law in the sense in which generalization is above defined: thus Kepler's empirical generalizations are known as his laws; while the word theory is used by some in a sense more nearly accordant with its derivation, as a principle or system which affords a *point of view* of a large number of facts and generalizations at once. If I understand Dr. Williamson's essay aright, he usually employs the term in this sense, and as not necessarily entailing any hypothesis at all, although he sometimes uses it in the sense of "generalization" as above defined.

The doctrines propounded by Dalton and adopted and extended by Berzelius include the three kinds of proposition, viz. generalization, convention, and hypothesis, and may be thus expressed:—

*Generalization.*—The quantitative composition of any homogeneous body may be represented with a considerable degree of accuracy (usually involving less error than that due to imperfection of instruments, impurity of specimen, &c.) by taking and comparing simple multiples of certain fixed numbers attached respectively to the name of each elementary substance occurring in (*i. e.* obtainable from) the body in question.

*Convention.*—This quantitative composition may be represented more briefly still by a system of symbols and suffixes, where the symbols indicate the names of the elements present, and the suffix applied to any symbol indicates the multiple of the fixed number mentally associated with that symbol which is to be taken in the particular compound under consideration. This amounts to the convention that all compounds are expressible by the general formula

$$A_a \cdot B_b \cdot C_c \cdot D_d \cdot \dots \cdot X_x \cdot Y_y \cdot Z_z,$$

where A, B, C, D . . . represent the symbols of the known elements, and *a, b, c, d . . .* any integer or zero.

*Hypothesis.*—The observed facts summed up in the generalization and expressed symbolically by the convention may be accounted for by supposing that matter is made up of indivisible portions of different kinds (atoms), and that the atom of each kind (elementary atom) has a weight proportionate to the fixed number attached to that element—a compound body being made up of a number of clusters of atoms (molecules), where the nature and number of atoms of each given kind in a cluster is denoted by the symbols and attached suffixes occurring in the formula of the body in question.

The term atomic hypothesis, therefore, as I understand it and have employed it, includes two ideas, viz. the expression of some proposition incapable of direct proof, and, secondly, a particular

proposition of this kind relating to the nature of matter; and when it is stated that the atomic hypothesis is unnecessary, what is meant is that in chemical investigation, where the object is to discover new facts, properties, substances, or reactions, to connect such discoveries with previous ones, to furnish the foundation for prediction by means of induction, to systematize and correlate the results obtained, &c., neither of these two ideas is in any way involved.

It does not, however, follow that because one does not see the necessity of admitting Dalton's hypothesis, one is therefore debarred from employing the generalization and convention of which he was the distinguished originator, nor that one is guilty of inconsistency in so doing; but it does follow, if any one makes the statement that the atomic theory is involved in Dalton's generalization and that this generalization has no existence apart from the atomic theory, that he is employing the term atomic theory in a sense different from that in which I have used the phrase atomic hypothesis.

When the developments that have been made on Dalton's original conceptions are closely examined, it is noticeable that an entire change of idea in many respects has taken place, so that the term atom is now employed to mean something very different from what it meant when Dalton first introduced the word into chemistry. One of the objections urged in my paper against the use of the language of the atomic hypothesis is that this language is practically ambiguous, owing to the gradual alteration that has taken place in the meaning of the terms, an alteration carried so far by Dr. Williamson as to lead to the distinct denial of the necessity of admitting the very proposition expressing Dalton's acceptance of the term atom, viz. that matter is made up of small *indivisible* portions. For the sake of clearness, therefore, it does not seem desirable that the terms involved in this admittedly unnecessary hypothesis should be retained and used in senses often opposed to those in which they were at first employed. It may be urged that no other single word exists capable of expressing the different ideas conveyed by the term *atom* as *now* employed; and grammatically speaking, there appears to be no reason why the word atom should not indicate a *minimum ratio* or *indivisible proportion* as much as an *indivisible fragment of matter*; still it does not seem philosophical to use terms which not only are easily susceptible of double meanings, but which must necessarily entail ambiguity; and in point of fact there is no actual necessity to use such terms, inasmuch as all the ideas involved can be conveyed in other and more definite language.

It being thus shown that what Mr. Atkinson actually dis-

cusses and refers to in his paper is something entirely different from the ideas embodied and principles laid down in what he professes to criticise, the reply to each of his remarks becomes very simple.

Thus, first, Mr. Atkinson considers that either the experimental numbers obtained in the quantitative analysis of a series of bodies composed of the same elements in different proportions (as, for instance, the tungsten chlorides) must be employed, or else the atomic theory is, "unconsciously, no doubt," assumed. It hence appears that Mr. Atkinson has missed the essence of the paper he attempts to criticise, viz. the distinguishing between the use of symbols to express with more or less accuracy observed facts, and the use of an hypothesis to explain these facts: in assigning a formula to a body from the results of quantitative analysis, Dalton's generalization only is involved, his hypothesis having nothing whatever to do with the matter. If what has been already said is not sufficient to convince the reader that Dalton's hypothesis (or, rather, modification of Epicurus's hypothesis) as to the constitution of matter is the *result* and not the *cause* of the generalization ordinarily known as the law of multiple proportions, no further remarks could have that effect.

If Mr. Atkinson's line of argument were correct, it would be impossible to assign any formula to any thing; for on repetition of an analysis, slightly varying numbers are obtained; the formula which expresses one analysis accurately would not express another; and there is no reason why one should be taken rather than the other. Again, does each so-called "experimental law" (generalization), which is not proved with mathematical accuracy by experiment, involve the conception of some theory whereby the experimental differences are rendered negligible? What theory, for example, is it without which the law of illumination being inversely as the square of the distance, has no real existence? or what theory is involved in the experimental generalization that an electric current is equal to the electromotive force divided by the resistance, or in the generalization that the volume of a gas is inversely as the pressure to which the gas is subjected?

Precisely the same argument is applicable to Mr. Atkinson's objection, that experiment does not prove that two volumes of each of the vapours of several compounds containing the same element contain *exact* multiples of one volume of the vapour of that element; no experimental law, *i. e.* generalization, ever is exactly proved; the generalization simply records approximately the result of any experiment, the approximation being closer the more carefully disturbing causes are eliminated.



Mr. Atkinson says that the definition given of a combining number does not always lead to the number in use, instancing ferric and aluminic chlorides as cases in point. It is almost superfluous to point out that when the combining number is defined as the minimum number of grams of an element contained in two volumes of the homogeneous vapour of any of its compounds, the examination of only one vapour is not sufficient to point out a *minimum*; thus, if benzene only were examined, the combining number of carbon would be found to be 72; if several compounds cannot be examined, then the combining number deduced from only one or two is liable to be diminished by arguments drawn from chemical analogy &c. In the case of aluminium, Mr. Atkinson is rather unfortunate in his example; for the experiments of Buckton and Odling tend to show that the formulæ of aluminic methide and ethide are respectively  $\text{AlMe}^3$  and  $\text{AlEt}^3$ , and hence that the combining number is 27.5 instead of 55 as deduced from the chloride alone.

Mr. Atkinson says that the rule given for the determination from its specific heat of the combining number of an element that does not yield a sufficient number of gasefiable compounds to allow of the application of the above rule is "even more fallacious;" but he does not apply it correctly in the examples he gives: the rule is deduced from the relations between the specific heat in the *solid state* of the elements "mercury, sulphur, selenium, tellurium, bromine, iodine, phosphorus, arsenic, antimony, bismuth, tin, osmium, zinc, and probably others." Mr. Atkinson, however, applies it to oxygen and chlorine, which are not usually regarded as solids\*—and instances, as exceptional cases which show that the data on which the rule is founded are too variable to justify reliance, boron, carbon, iron, and aluminium. As regards the first two, they are not mentioned in connexion with the rule; and the fact of their not agreeing with it no more vitiates it than the irregular behaviour of carbon dioxide when near its condensing-point vitiates Marriotte's law; while aluminium is perfectly regular, the combining number deduced from the methide being taken. As to iron, it has been pointed out in the last paragraph that the combining number cannot be fixed from one or two vapours only; and hence this metal cannot be cited as an exception.

Mr. Atkinson considers that the extension of the term combining number to relative numbers *approximately* equal to  $\frac{6.6}{8}$  is an admission of the "worthlessness of the rule." If the cir-

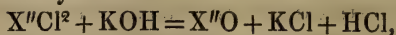
\* Mr. Atkinson not only does not take the best determinations of the specific heats of oxygen and chlorine, but he takes the specific heats under *constant volume* instead of under *constant pressure*.

cumstance that experimental generalizations can only express observed facts with approximate accuracy is to be taken as evidence of their worthlessness, the sooner we cease to waste time by attempting to systematize phenomena and show their correlation by the employment of experimental generalizations the better. Even astronomical observations do not always exhibit a mathematical agreement with the generalizations that express them with a great degree of approximation; and if the generalizations are "worthless," what becomes of the theory of gravitation (*i. e.* the developed hypothesis) which rests on them?

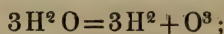
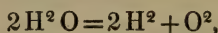
The remarks of Mr. Atkinson on the applicability of the term "atom" to express the *observed facts* that 16 parts by weight of oxygen or 12 of carbon is the minimum relative weight or smallest proportion in which it exists in compounds have been already discussed. The term atom is not used in the sense of a proportion or relative number anywhere throughout my paper; it is used in reference to a particular *hypothesis*, which is not identical with the facts from which it takes its origin. *Apropos*, Mr. Atkinson considers that "Dr. Williamson would probably be astonished to learn that he looked upon an atom as a pure number." Now it is not asserted that Dr. Williamson always employs the term in this particular defined sense; but when he distinctly states that the notion of an atom being indivisible is a question not raised in chemistry by any evidence whatever, and that he knows not whether atoms are really ultimate atoms at all, whether spherical, regular, or irregular, or whether they are not vortices, it is somewhat difficult to see in what connexion Dr. Williamson views them other than as simple numerical quantities. Mr. Atkinson does not seem to be quite clear in his own mind as to the meaning of the phrase "being possessed of dimensions in space, mass, and time." Perhaps he is not aware that these terms are fundamental conceptions involved in any proposition which relates to energy or to force, and that therefore they are necessarily connected with any question in which *weight* is involved.

Mr. Atkinson inquires what is the meaning of an equivalent quantity of a radical, which radical is defined to be one or more symbols and suffixes transferable from one formula to another; and he appears to be amused at the idea of valency being a function of an assemblage of symbols. Nevertheless this is precisely what valency is; it is a term expressing certain differences between symbols, such differences being deduced from the symbolic representation of *observed reactions*. Thus, to take the question of so-called direct and indirect combination referred to by Mr. Atkinson, experiment shows that caustic potash is denoted by the formula OKH (KOH, KHO, &c.); if this substance, when

treated by an appropriate agent undergoes a reaction of the kind expressed by the symbols



such a reaction is alluded to and understood by the phrase "O is a bivalent symbol, and H and K univalent ones in formulæ where caustic potash and its derivatives are referred to;" in other words, the valency of a particular radical is a particular function which is fixed by the nature of the reactions which the bodies whose formulæ contain that radical are capable of undergoing. As to the phrase "equivalent quantity of a radical," it is self-evident that it is an elliptical expression. To take a case as an example, the reactions of water show that O is a bivalent symbol and H a univalent one in such reactions; hence the relative equivalents of the radicals O and H are  $\frac{16}{2}$  and  $\frac{1}{1}$ , or 8 and 1. Thus, when water is electrolyzed, one or both of these reactions ensue,



that is, the weights of hydrogen and oxygen, or hydrogen and ozone, are always found to be in the proportions, respectively,

$$2(1 \times 2) \text{ to } (8 \times 2) \times 2,$$

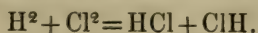
or

$$3(1 \times 2) \text{ ,, } (8 \times 2) \times 3.$$

Mr. Atkinson desires that I should account for the differences between isomeric bodies. This is a problem somewhat beyond the province of the paper which Mr. Atkinson is discussing, the object of which was to point out the connexion between a particular hypothesis and certain facts expressed in symbols. In that paper it was very briefly pointed out that the different chemical reactions of isomeric substances are readily expressed by different modes of dissection of the same formula. The limits of the paper as to length did not permit a full discussion of the relations of the atomic hypothesis to isomerism; but I would now ask, Does that hypothesis explain isomerism? or is it not rather opposed to what little is known on the subject? On this hypothesis, isomeric molecules consist of the same number of the same atoms, but in different relative positions; then, either the atoms are rigidly connected, or they have a limited amount of mobility among themselves: they cannot be perfectly free to move in all directions; otherwise the molecule would not hold together. Now all our notions of force, and the observed transformations of potential into actual energy, or the reverse, which take place in chemical reactions, are opposed to the first suppo-

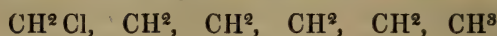


sition, inasmuch as the motion which appears or disappears during a reaction can only be considered to be derived from, or imparted to, the atoms themselves; *i. e.* *intramolecular* motion of variable amount must exist. Since, then, the atoms must have a limited amount of motion (vibratory, rotatory, &c.) among themselves, what are the differences in the rate or kind of motion corresponding to the chemical differences between (for example) the three kinds of hydrogen existing in ethylic alcohol in the groups  $\text{CH}^3$ ,  $\text{CH}^2$ , and  $\text{OH}$ ? Before the atomic hypothesis can be said to *explain* isomerism, some connexion must be made out between these different rates or kinds of intramolecular motion and the chemical and other properties of the isomerides; for isomerides differ not only in mechanical properties, such as specific gravity, melting-point, &c., and in chemical reactions, but also in the amount of heat given out on combustion of equal weights of the substances respectively; *i. e.* each isomeride represents a different amount of potential energy; and the same holds in the case of allotropic modifications, *e. g.* the carbon and phosphorus allotropes. The mere hypothesis of the *existence* of atoms is insufficient to explain these observed facts; whilst, even if supplemented by a number of further hypotheses as to the rate and character of intramolecular motions in given instances, a satisfactory explanation of chemical phenomena by these means does not seem probable. Thus, granting that the substitution of a chlorine atom for a hydrogen atom in one molecule is accompanied by a given change in the motion, either of that atom alone, or in the motion of the other atoms in the molecule as well, it does not appear that it will be possible to predict what will be the changes of motion that take place when the substitution of the chlorine atom for the hydrogen atom occurs in another molecule, or even in a different portion of the same molecule. For instance, granting that the substitution of chlorine for hydrogen is attended by a given change of motion, the inverse substitution must be accompanied by the opposite change. Now, when a molecule of hydrogen is acted on by a molecule of chlorine, in one molecule an atom of hydrogen is replaced by one of chlorine, and in the other an atom of chlorine is replaced by one of hydrogen. These complementary changes, therefore, should cause, on the whole, no difference as to the ultimate amount of motion in the two original and the two resulting molecules; but as a matter of fact there is such a difference observed (*i. e.* potential energy becomes actual) when hydrogen and chlorine act on one another in accordance with the equation

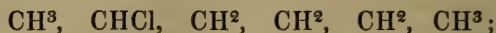


Again, to take a simple case, when chlorine acts on hexyl-

hydride, the two resulting monochlorides are (Schorlemmer)



and



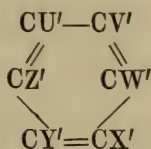
i. e. one represents a primary, the other a secondary alcohol. What are the differences in the changes of motion that ensue on the replacement of a hydrogen atom by a chlorine atom, according as this atom is one of the terminal or of the penultimate group? and why is not one of the more central groups affected? So far from the atomic hypothesis giving an explanation of chemical phenomena relating to isomerism, the explanation of the vast majority of facts by this means has never yet been even dreamed of.

It might be fairly urged against the symbolic system that it does not at present include in the meanings of the symbols the amount of energy converted from the potential to the actual state, or *vice versa*, in any given reaction; but this is a point out of the cognizance of my paper, which only treated of those things that *are* represented by these symbols.

It is sometimes stated that the existence of atoms (or at least of molecules) is demonstrated by the consideration of the mechanical properties of gases and other physical facts: but is there not a *petitio principii* here? Granting the existence of molecules, certain observed properties of gases may be explained, just as the generalization of multiple proportions is explained by this supposition; but that is no proof of the existence of molecules. Granting their existence, from certain physical considerations their size may be approximately calculated; but that is no proof of their existence, any more than the circumstance that certain electric phenomena are explainable by, and certain results deducible from, the hypothesis of the existence of an imponderable fluid is a proof of the existence of such a fluid.

When it is urged in favour of the retention of such terms as atom, &c., that the advances in chemistry during the last forty years are due to the atomic theory, another fallacy is introduced. It does not follow that because a wire is useful in keeping a young sapling erect, therefore the wire will be beneficial when the plant is strong enough to dispense with its aid; nay, the wire is prejudicial, as, if retained, it warps the tree from the perpendicular and causes it to grow distorted. But besides this the question arises, What is meant by the term atomic theory in this case? If it be used to designate what has been styled in my paper as the atomic hypothesis, the truth of the proposition may be doubted, at least in very many cases. Thus it is questionable whether the chemists who employ Kekulé's views in their researches on aromatic compounds use the symbolic repre-

sentations of these views as necessarily involving the notion of indivisible portions of matter. True, the word "atom" may be employed, but not in the Daltonian sense of the term, any more than it is employed in that sense by Dr. Williamson. Kekulé's propositions in fact amount to these: a large number of organic bodies are found to undergo changes and produce reactions which are expressible by the generalization and convention that the formulæ of these substances are all capable of expression by the *general dissection*



where U', V', W', X', Y', Z' represent either H or some other univalent radical respectively. The extensions of knowledge brought about since, and in consequence of, the propagation of his views are apparently referable much more to the employment of symbols to indicate briefly and comprehensively a host of facts (or, what is much the same thing in principle, to the use of comprehensive terms such as "atom" in senses not involving any hypothesis, but having only reference to generalizations and conventions), than to the influence of Dalton's modification of Epicurus's hypothesis as to the constitution of matter.

If, however, the term atomic theory be not used in this defined sense, but be employed, as by Dr. Williamson, to indicate chemical philosophy generally, with the exclusion of Dalton's fundamental notion as to the existence of indivisible portions of matter, then the statement that the advances in chemistry are due to the atomic theory is a virtual admission of my points, viz. that the advantages arising from the *hypothesis* of Dalton (as opposed to his *generalization* and *convention*) are much overrated, and that the discussion of hypotheses in connexion with phenomena is of comparatively little benefit to the chemist until he can arrive at some one hypothesis sufficiently comprehensive to take in *all* the phenomena observed by him. Now it may be possible for the atomic hypothesis, with the aid of vast numbers of subsidiary postulates and hypotheses, to explain all existing facts, more especially those concerning the relations between energy and chemical action; but alone it certainly cannot do so, and as yet it has not been shown to explain (even with such additions) the few isolated facts in this latter field at present known to us.

The examination of this field constitutes the chemistry of the future. As yet, however, this is barely regarded as even form-



ing a part of chemistry, the scope of which branch of science as ordinarily understood might be defined thus:—"Chemistry is that branch of the study of matter which takes cognizance of the changes produced by the action of bodies on one another without involving any quantitative measure of energy of any kind, but only the observation of particular conditions (*e. g.* temperature, pressure, &c.)." Chemistry will in time mean something wider than this; it and physics will be regarded as coextensive, and identical in kind though not in degree,—physics taking cognizance of those actions where the change in the properties of the bodies is small or gradual, chemistry of those which are greater or more rapid; so that the chemistry of the future may be defined as "that branch of the study of surrounding objects which relates to their mutual actions on one another so as to produce fresh bodies differing in properties from the original ones, such changes being accompanied by a measurable transformation of potential into actual energy, or *vice versâ*"—physics being, in short, a subsection of chemistry, and a necessary preliminary to its study.

Until a copious store of knowledge be gained in this almost untrodden realm, the materials are absent on which to superinduce an hypothesis sufficiently comprehensive to deserve the name of theory (in the ordinary acceptation of that term, *i. e.* as one speaks of the theory of gravitation, the developed hypothesis). But what little is known indicates that the atomic hypothesis alone is inadequate to take in all phenomena; on the other hand, it is admitted by many chemists, and notably by Dr. Williamson, that conceptions involved in this hypothesis are "not raised in chemistry by any evidence whatever;" while experience proves that the language founded on this hypothesis has so altered in meaning as now to be occasionally applied in senses contradictory of the original acceptations; so that, in fine, I see no reason for altering the final conclusions come to in the paper examined (?) by Mr. Atkinson.

Laboratory, St. Mary's Hospital, W.,  
June 12, 1872.

LXIII. On a New Hygrometer. By A. DE LA RIVE.

To the Editors of the *Philosophical Magazine and Journal*.

GENTLEMEN,

Geneva, June 1, 1872.

HAVING just seen, in volume xx., No. 132, of the 'Proceedings of the Royal Society,' the description of a "new hygrometer" by Mr. Wildman Whitehouse\*, allow me to state that an instrument founded on exactly the same principle was

\* See p. 538 of the present Number.

proposed by me in 1825. My paper on the subject first appeared in the *Bibliothèque Universelle* of Geneva (vol. xxviii. p. 285); and the following extract of it was published the same year by Gay-Lussac in the *Annales de Chimie et de Physique* (vol. xxx. p. 87).

I remain, Gentlemen,

Yours &c.,

A. DE LA RIVE.

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*Translated from the Annales de Chimie et de Physique* (vol. xxx. p. 87).

"I plunge the bulb of a delicate thermometer into concentrated sulphuric acid, and then suddenly withdraw it with a slight shake so as to leave only a thin film of acid on the surface of the bulb. The thermometer immediately rises several degrees; and after remaining an instant stationary, it begins to fall. I next determine the number of degrees the thermometer rises, under the same circumstances, when the atmosphere is completely saturated with moisture; the difference between the two results will furnish the exact relation between the tension of the vapour contained in the atmosphere at the moment of my observation and its tension when the atmosphere is in a state of perfect saturation. For instance, the thermometer, at the moment of the bulb being introduced into the sulphuric acid, indicated  $12^{\circ}$  Réaumur. On being withdrawn and exposed to the air it rose to  $25^{\circ}5$ , or  $13\frac{1}{2}^{\circ}$ . Placed under a receiver in an atmosphere of perfect saturation at the same temperature of  $12^{\circ}$  it rose to  $27^{\circ}$ , or  $15^{\circ}$ . Hence the ratio of  $13\frac{1}{2}$  to 15 expresses the relation that exists between the tension of the vapour contained in the air and the tension of saturation for a temperature of  $12^{\circ}$ . Now the ratio of  $13\frac{1}{2}$  to 15 is equal to that of 90 to 100; and it will be found, by referring to Gay-Lussac's Table, published in the *Traité de Physique Expérimentale et Mathématique* of Biot, that the degree of De Saussure's hygrometer which corresponds to the tension of 90 is 95.43. During the above experiment the reading of the same hygrometer was 95.50.

"For any other temperature than that of  $12^{\circ}$  R., and for the same degree of De Saussure's hygrometer, the rise of the thermometer will be proportional to the temperature of the atmosphere, since the quantity of aqueous vapour the air is capable of containing depends mainly on its degree of temperature. It would therefore appear to be necessary to ascertain the variation of temperature, produced by air in a state of saturation, for each degree of the thermometer, were it not that a series of experi-

ments have convinced me that it was sufficient for all practical purposes to determine the rise of the thermometer, in the case of a saturated atmosphere, for two extreme points only, such as  $0^{\circ}$  and  $20^{\circ}$  R., and to distribute this difference equally between the intermediate degrees. It also appeared to me that the variations of the thermometer, at the moment of its being withdrawn from the acid, were proportional to the tensions of the vapour at the above temperatures.

"It might appear probable at first sight that, however small the quantity of vapour contained in the atmosphere, there would always be a sufficient quantity of moisture to saturate the acid film spread over the surface of the bulb, and consequently to develop in every case an equal quantity of heat. My answer to that objection is, that there appears to exist a sort of opposition between the affinity of the acid for moisture and the tendency of water to remain in a state of aqueous vapour, which increases in proportion to the dryness of the atmosphere. It follows that the greater the quantity of moisture contained in the atmosphere the greater the rapidity with which it will be condensed by the acid, and consequently the greater the heat developed. The thermometer in each case will continue rising until the cooling influence of the surrounding air becomes sufficient to neutralize the quantity of heat due to the condensation of moisture, and the moment at which that effect is produced must depend upon the greater or less quantity of vapour the atmosphere contains."

LXIV. *On the History of the Second Law of Thermodynamics, in reply to Professor Clausius.* By Professor TAIT\*.

PROFESSOR CLAUSIUS seems to forget that it was he who raised this discussion, and that he used the words "Absichtlichkeit," "sehr geschickt abgefasst," &c. Such language may amaze and amuse; it is too reckless to produce irritation. Does Professor Clausius seriously suppose that Thomson, Clerk-Maxwell, and myself have been deliberately, by suppression and by special pleading, attempting to deprive him of his just claims? So desirous was I to do him full credit that, when he (in correspondence) objected to a remark which is certainly in substance correct, and which would otherwise have appeared in my little work, I requested a friend who is thoroughly acquainted with Professor Clausius's papers to rewrite for me the greater part of the pages bearing on them.

I am happy to find that Professor Clausius has now moderated his tone, and that he does not repeat his pretensions to a share

\* Communicated by the Author.



in the Dissipation of Energy. And, though I gladly comply with his desire to leave to future generations the value of "innere Arbeit" and of "Disgregation," I should much like to know in what respect the former term is an improvement on the name "Latent Heat" which was given by the discoverer himself.

But I think it would be a pity to leave to the future the discussion of the other point at issue, since so much depends upon Professor Clausius's interpretation of his own words. The question is, "Who first *correctly* adapted Carnot's magnificently original methods to the true Theory of Heat?" Nothing was wanting for this but a sound Axiom: the method was already provided; and the brothers Thomson had (in 1848 and 1849) recalled it to the attention of the scientific world (Professor Clausius included) by deducing from it an absolute definition of temperature, and the effect of pressure upon the melting-points of solids.

Now it is one thing to rush into print with a proof which has afterwards to be explained and patched up, and quite another thing to wait till one hits on a complete and irrefragable demonstration.

The following are, as far as I can see, the words to which Professor Clausius refers as implicitly containing his Axiom, which is nowhere explicitly stated in his first paper. They do not contain the phrase "von selbst," to which he assigns so important and extensive a meaning. "Durch Wiederholung dieser beiden abwechselnden Processe könnte man also, ohne irgend einen Kraftaufwand oder eine andere Veränderung, beliebig viel Wärme aus einem *kalten* Körper in einen *warmen* schaffen, und das widerspricht dem sonstigen Verhalten der Wärme, indem sie überall das Bestreben zeigt, vorkommende Temperaturdifferenzen auszugleichen und also aus den *wärmeren* Körpern in die *kälteren* überzugehen." I still fail to reconcile this with the thermoelectric phenomena to which I referred, in which certainly part of the heat has the very opposite "Bestreben" to that which Professor Clausius assumed to be universal. And I think that Thomson has done mischief as regards scientific history, by giving Professor Clausius undue credit, and unwarrantably representing these words as containing the Axiom, "It is impossible for a self-acting machine, unaided by any external agency, to convey heat from one body to another at a higher temperature." Moreover Thomson is certainly mistaken when he asserts that even this is equivalent to his own Axiom.

P.S.—I have just seen the 4th Heft of Poggendorff's *Annalen*, recently published. I shall probably refer more in detail (on

another occasion) to the extraordinary statements there made by Professor Wüllner. At present I must content myself with the remark that he does not seem to have read even Clausius's paper on Thermoelectricity; for in it Thomson's priority as regards that subject is admitted.

LXV. *On Electrolysis, and the Passage of Electricity through Liquids.* By G. QUINCKE.

[Continued from p. 375.]

§ 55.

IN liquids which are electrolyzed, the quantity of electricity passing through them consists of two parts. The one consists of the particles of electricity which are given from one molecule to the adjacent one, which pass through the liquid with what is called metallic conductivity; the other consists of the particles of electricity which are carried forward in the liquid by material molecules\*. Now the first part, as compared with the second, is extraordinarily small—so much so, in fact, that the very existence of this metallic conductivity is often denied†. But with a sufficient degree of approximation we may regard as the whole quantity of moving electricity that which, adhering to the material molecules, moves simultaneously with them.

If  $p$  is the number of molecules of salt contained in the unit of volume of the liquid,  $\alpha$  and  $\alpha'$  the equivalent-weights of the partial molecules, then

$$\alpha = pa, \quad \alpha' = pa', \quad . . . . . (13)$$

and equations (8) pass into

\* Conf. Kohlrausch and Weber, "Electrodynamische Maassbestimmungen," *Abh. d. K. S. G. d. Wissensch.* vol. v. p. 272. Faraday, *Phil. Mag.* S. 4. vol x. (1855) p. 107.

† From the simple fact that no substance is a perfect insulator, it follows that all must have some conducting-power of the same kind as the metals. No solid salt which is not decomposed by the electrical current is a complete insulator; and hence, if the opinion above expressed were incorrect, we should have to make the surprising assumption that the mere change of state of aggregation in one series of compounds (electrolyzable) destroyed the property of possessing a so-called metallic conductivity, while in other (non-electrolyzable) compounds such a change of the conducting-power could not occur (compare "On the Conducting-power of Chloride of Lead and Oxide of Lead," Buff, *Liebig's Annalen*, vol. c. (1859) p. 285). It is probable that Faraday's law does not hold with entire strictness; but the deviations are so small that they lie far within the limits of unavoidable errors of observation.

$$\left. \begin{aligned} \frac{M}{a} &= i \frac{p}{\lambda} C \epsilon, \\ \frac{M'}{a'} &= i \frac{p}{\lambda} C \epsilon'. \end{aligned} \right\} \dots \dots \dots (14)$$

$\frac{M}{a}$  is then the number of partial molecules which pass to the cathode,  $\frac{M'}{a'}$  the number of partial molecules which pass to the anode in the unit of time. Each of these particles is the bearer of a quantity of electricity  $\epsilon$  or  $\epsilon'$ ; and therefore through the section of the thread of liquid there pass in the unit of time the quantities of electricity

$$\left. \begin{aligned} \frac{M}{a} \epsilon &= i \frac{p}{\lambda} C \epsilon^2, \\ \frac{M'}{a'} \epsilon' &= i \frac{p}{\lambda} C \epsilon'^2. \end{aligned} \right\} \dots \dots \dots (15)$$

According to the sign of  $\epsilon$  or  $\epsilon'$  they may be positive or negative, and have in general different magnitudes.

In electrical currents in metals it is assumed, on the contrary, that the same quantity of positive electricity flows in one direction through a definite section, as of negative electricity in the opposite direction\*. That, notwithstanding this, no difference can be perceived† between the action of the same electrical current whether it flows through an electrolyte or a metal, is in complete accordance with other phenomena—for instance, the discharge of the Leyden jar, where no difference is observed, whether the quantity  $+e$  flows in one or  $-e$  in the other direction through the conductor. If  $i$  be the intensity of the current, in a metallic conductor, this means that through a section of the metal the quantity  $+i$  of positive electricity flows in one, and  $-i$  in the other direction in the unit of time. The whole quantity of positive and negative electricity which traverses the same section in the unit of time is therefore  $2i$ . Hence the entire quantity of electricity carried in the unit of time through the section of the thread of liquid is  $=2i$ , and we have

$$2i = \frac{M}{a} \epsilon + \frac{M'}{a'} \epsilon' = i \frac{p}{\lambda} (C \epsilon^2 + C' \epsilon'^2), \dots \dots (16a)$$

where the signs of  $\epsilon$  and  $\epsilon'$  need not be taken into account.

If from equations (13) and (14) the values of  $M$ ,  $M'$ , and  $\alpha$ ,

\* Kirchhoff, *Pogg. Ann.* vol. lxxviii. p. 509.

† Kohlrausch, *Pogg. Ann.* vol. xcvi. (1856) p. 559.



$\alpha'$  be substituted in equation (9), then

$$\frac{m}{a} = \frac{m'}{a'} = i \frac{p}{\lambda} (C\epsilon - C'\epsilon'), \quad . \quad . \quad . \quad (17a)$$

where the negative sign at  $\frac{m'}{a'}$ , for the partial molecules liberated at the anode, is omitted.

$\frac{m}{a}$  is the number of partial molecules separated at the cathode in the unit of time, each of which is the carrier of a quantity of electricity  $\epsilon$ . To the metal of which the cathode is formed the quantity of electricity  $Q = \frac{m}{a} \epsilon$  is thereby communicated.

In like manner, to the metal of the anode, from the  $\frac{m'}{a'}$  partial molecules separated at it, the quantity of electricity  $Q' = \frac{m'}{a'} \epsilon'$  will be given up; and we have

$$\left. \begin{aligned} Q &= \frac{m}{a} \epsilon = i \frac{p}{\lambda} (C\epsilon - C'\epsilon')\epsilon, \\ Q' &= \frac{m'}{a'} \epsilon' = i \frac{p}{\lambda} (C\epsilon - C'\epsilon')\epsilon'. \end{aligned} \right\} . \quad . \quad . \quad (18a)$$

These quantities of electricity, again, according to the sign of  $\epsilon$  or  $\epsilon'$  may be either positive or negative.

If  $J$  be the intensity of the current in the metal wire which carries the electrical current either to or from the liquid, from the remarks at the commencement of this section the increase of electricity which the first section of the metallic cathode plate experiences in the unit of time is  $= -2J$ , and the increase of the last section of the metal anode plate is  $= +2J$ .

If there be no accumulation of free electricity in these metal electrodes, then must

$$Q - 2J = 0, \quad Q' + 2J = 0. \quad . \quad . \quad . \quad (19a)$$

In this reasoning it is assumed that entire molecules of *one* salt only are decomposed. If several salts and at the same time the solvent are decomposed, we have, instead of equations (16a), (17a), (18a), and (19a), if we discriminate the various constants for the different decomposed substances by the index  $r$ , the more general equations

$$2i = \sum_r \left( \frac{M_r}{a_r} \epsilon_r + \frac{M'_r}{a'_r} \epsilon'_r \right) = \frac{i}{\lambda} \sum_r p_r (C_r \epsilon_r^2 + C'_r \epsilon'^2_r), \quad . \quad (16)$$

$$\frac{m_r}{a_r} = \frac{m'_r}{a'_r} = \frac{i}{\lambda} p_r (C_r \epsilon_r - C'_r \epsilon'_r), \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

$$\left. \begin{aligned} \sum_r Q_r &= \sum_r \frac{m_r}{a_r} \epsilon_r = \frac{i}{\lambda} \sum_r p_r (C_r \epsilon_r - C'_r \epsilon'_r) \epsilon'_r, \\ \sum_r Q'_r &= \sum_r \frac{m'_r}{a'_r} \epsilon'_r = \frac{i}{\lambda} \sum_r p_r (C_r \epsilon_r - C'_r \epsilon'_r) \epsilon'_r, \end{aligned} \right\} . \quad . \quad . \quad (18)$$

$$\sum_r Q_r = 2J, \quad \sum_r Q'_r = -2J, \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

where the  $\sum_r$  are to be extended to all the  $r$  various chemical compounds which are simultaneously decomposed in the liquid.

These equations hold even if a complete molecule of a chemical compound is not decomposed by the electrical current. If we designate by  $\rho$  the corresponding value of  $r$  for a complete molecule, we have, since both partial molecules must move with the same velocity, according to equation (7),

$$C_\rho \epsilon_\rho = C'_\rho \epsilon'_\rho; \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

or the two magnitudes  $\epsilon_\rho$  and  $\epsilon'_\rho$  must have the same sign. From equations (20), (14), and (17) we get then

$$\frac{M_\rho}{a_\rho} = \frac{M'_\rho}{a'_\rho}, \quad \frac{m_\rho}{a_\rho} = \frac{m'_\rho}{a'_\rho} = 0.$$

As, moreover, in every volume of the whole liquid there must be equal quantities of positive and negative electricity,

$$\sum_r p_r (\epsilon_r + \epsilon'_r) = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

where the sum is to be extended to all the  $r$  substances or salts present in the liquid, to those even whose total molecules are not decomposed.

If the intensity of the current in the column of liquid is the same as in the metallic part of the conduction, as experiment shows (compare § 69), then

$$i = J. \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

When the same current is sent through several liquids adjacent to one another so that it traverses one liquid after the other, no free molecules appear at the boundary of the various electrolytes, provided the partial molecules liberated at the cathode of one liquid can again form a chemical compound with those liberated at the anode of the adjacent liquid.

From this follows Faraday's celebrated electrolytic law\*, that the number of equivalents (partial molecules) liberated at the electrodes in the unit of time is a measure for the quantity of

\* Faraday, Experimental Researches, § 377, pp. 504 & 505, 783 *et seqq.*





thus, in spite of this difference in the mode of conduction, the electrolytes must be subject to the same laws of the distribution of the current as the metals. Hence both Ohm's law and Kirchhoff's propositions on derived currents must hold, and the potential of free electricity in the case of a linear conductor must be constant within the same section.

Ohm's law has been so much tested for electrolyzable liquids with an undivided circuit that it can be taken as sufficiently proved.

Yet in one particular case the laws of divided circuits seem to experience an exception. Poggendorff\* found that the resistance of a very thin platinum wire stretched in the axis of a vertical glass cylinder of  $3\frac{1}{2}$  inches diameter was unaltered when this cylinder was filled to a height of  $6\frac{2}{3}$  inches with dilute sulphuric acid. There was no trace of a lateral extension, or of a division of the current between the metal and the liquid.

Jacobi subsequently† made similar experiments with German-silver or platinum wires which were stretched on a wooden trough 20 inches long by  $3\frac{1}{4}$  broad and 4 high, lined with marine glue. In one case, when the trough was filled with sulphate of copper, a very slight diminution of the resistance of the German-silver wire was observed by the aid of Wheatstone's bridge; in another experiment with German-silver wire and platinum wire a diminution of the resistance could not be detected by the same method; on the other hand, at the end of the German-silver wire towards the negative pole of the circuit there was a deposition of copper, while at the other end the wire was attacked and even eaten through. The deposition of copper and the corrosion were strongest at the ends. The platinum wire showed only a trace of a copper precipitate at the end turned towards the negative pole.

It has therefore been supposed, and Wiedemann has also expressed the idea‡, that this apparent irregularity in the division of the current between wire and liquid has its origin in a polarization at the limit of metal and liquid, produced by the electrochemical decomposition of the liquid, and that, when this polarization is wanting, part of the electric current actually does flow through the liquid.

The following experiments have confirmed this supposition.

A glass trough, 442 millims. in length, 60·6 millims. in breadth, and 62·5 millims. in height, was constructed of plate glass cemented with sealing-wax. In the middle of the bottom-plate a platinum wire of the same length and 0·076 millim. in diameter was

\* Pogg. *Ann.* vol. lxiv. p. 54 (1845).

† *Ibid.* vol. lxix. p. 181 (1846).

‡ Wiedemann, *Galvanismus*, vol. i. p. 138.

stretched, passing through the sealing-wax cement of the smaller sides of the trough to mercury-cups which were attached to the outside of the trough. By means of a Wheatstone's bridge the resistance of this platinum wire was compared with the resistance of a German-silver wire of almost the same dimensions and at a constant temperature. A very homogeneous brass wire 1000 millims. in length and 0.32 millim. in diameter served as measuring-wire. The current of a Grove's battery of seven pairs was passed through the divided circuit as long as was necessary for observing a deflection on the reflecting galvanometer; and the disturbing influence of thermocurrents was avoided by alternating observations with opposite direction of the principal current of the battery.

With this apparatus the resistance  $P$  of the platinum wire could be compared with that of the German-silver wire, known in mercury units, with accuracy to about one 5000th part.

The resistance was found to be

$$P = 16.207 \text{ m. u.}, \text{ or } P = 16.204 \text{ m. u.}$$

at  $14^{\circ}.4 \text{ C.}$ , according as the glass trough was full of air, or was filled with dilute sulphuric acid of the specific gravity 1.16 to a height of 49 millims. These numbers may be regarded as equal, taking into account the difficulty of maintaining at the same temperature the wire and the dilute sulphuric acid.

In another series of experiments the resistance of the platinum wire was determined in air, then after the trough had been filled to a height of 47.3 millims. with dilute sulphuric acid of the specific gravity 1.109, and finally after the dilute sulphuric acid had been removed from the trough by means of a siphon.

These three determinations at  $15^{\circ}.6 \text{ C.}$  gave

$$P = 16.280 \text{ m. u.}, \quad 16.278 \text{ m. u.}, \text{ and } 16.278 \text{ m. u.}$$

We see thus that the resistance of platinum wire remains unchanged whether there is air or dilute sulphuric acid in the glass trough. The same result was obtained when the resistance was determined on first closing the current after the sulphuric acid had been poured in.

The reason is, the immediate polarization of the platinum wire, the liberation of oxygen and hydrogen by the currents which branch off from the wire into the liquid.

Let  $A$  be the end of the platinum wire where the electrical current enters,  $B$  the end where the current emerges. After the current had passed for some time through the platinum wire, contact was broken and a freshly ignited platinum wire,  $C$ , was dipped in the sulphuric acid near the end  $A$  or  $B$  of the other wire. If the freshly ignited wire and the horizontal one in the glass trough were then connected by the wire of a delicate gal-

vanometer, a current was observed the direction of which in the liquid was from the ignited platinum wire C to the end A, or from the end B to the ignited platinum wire C. The ends A and B of the long platinum wire behaved thus towards the freshly ignited wire C like platinum wires the surfaces of which are covered with oxygen and hydrogen.

If the long platinum wire of the glass trough was replaced by a copper wire of the same length and 0.08 millim. in diameter, and instead of the dilute sulphuric acid a concentrated solution of pure sulphate of copper was used, the polarization at the boundary of the metal and the liquid was infinitely small; and now, when the solution of sulphate of copper was poured in, there was a diminution in the resistance of the copper wire. The resistance K of the copper wire was again compared, as described above, with the almost equal resistance of a German-silver wire, and was found to be, at  $15^{\circ}.5$  C.,

$$K = 2.783 \text{ m. u., or } 2.728 \text{ m. u.,}$$

according as the glass trough was filled with air, or to the height of 50 millims. with a sulphate-of-copper solution.

After the current of a Grove's battery of six elements had been passed for about an hour through the copper wire while surrounded by solution of sulphate of copper, the end A, where the current entered, close to the inside of the glass trough was eaten away, while the end B, where the current emerged, was coated with freshly precipitated copper. In the middle the copper wire was of the original thickness; towards the end A it was thinner, and towards B thicker. The decrease or increase was greater the nearer the part of the wire was to the end A or B, and the diameter of the wire at B was now 0.104 millim.

It follows from these experiments that an electrical current divides between metallic and liquid (electrolyzable) conductors, as Kirchhoff's laws require, and that in many cases the polarization produced by electrolysis causes only an apparent exception from this rule.

[To be continued.]

LXVI. *Water-Analysis and Water*. By J. ALFRED WANKLYN, Corresponding Member of the Royal Bavarian Academy of Sciences\*.

THE "ammonia process" of water-analysis which was brought out five years ago by Chapman, Smith, and myself, has now become of sufficient importance to repay a minute examination into its validity and special characteristics. Not

\* Communicated by the Author.



alone is the process applicable to drinking-water, but Dr. Angus Smith has used it in a most elaborate investigation of the atmosphere, and Dr. Ransome in investigating the breath in different diseases ; and I have in view a very wide and general use of it in physiological chemistry.

The "ammonia process" consists in oxidizing organic substances in a strongly alkaline solution, and measuring the ammonia yielded by oxidation under such conditions.

The oxidizing agent hitherto employed by us in working the process has been permanganate of potash, which is very convenient and manageable, and has the great advantage of not attacking the ammonia which is produced. An investigation of the action of other oxidizing agents in alkaline solution would repay the trouble of making it, and may possibly be undertaken at some future time.

We have submitted a large number of nitrogenous organic substances, exhibiting the utmost diversity of structure, to the action of strongly alkaline permanganate, and obtained ammonia as a product of the action. Quoting from a paper published by the Chemical Society (*vide* Journ. Chem. Soc. ser. 2, vol. vi. p. 170), "The compound ammonias of all kinds, the amides of the acids, such substances as piperine, hippuric acid, creatine, the natural alkaloids, albumen, gelatine, and uric acid evolve ammonia when treated in this way. Even so tough a substance as picoline, which, as is well known, is one of the most stubborn of organic compounds, yields ammonia when subjected to this treatment."

An elaborate investigation carried out with the express object of ascertaining if there were organic compounds containing nitrogen which yield no ammonia on boiling with the permanganate in alkaline solution, has given the following results :—

Nitro-compounds do not yield their nitrogen in the form of ammonia in this reaction. That such would be the case might almost have been concluded *à priori* ; for an oxidation is not calculated to convert nitric oxides into ammonia. It is satisfactory, however, to have direct experimental evidence. The case investigated was picric acid, which gave nitric acid on treatment with the permanganate, but no ammonia. Ferrocyanide of potassium failed to give ammonia, probably in virtue of its extreme toughness. Urea failed to give ammonia on oxidation in alkaline solution of permanganate. These were the only cases wherein organic nitrogenous substances failed to give ammonia on being boiled with permanganate of potash. The case of urea is particularly interesting. On inspection of its formula, it will be perceived that it contains less hydrogen than is re-

quired to supply the whole of its nitrogen with sufficient hydrogen to form ammonia. By assimilation of water it yields ammonia and carbonic acid; but by assimilation of oxygen it could not possibly yield up more than half of its nitrogen in the shape of ammonia, and, as I have said, has been found experimentally to yield up absolutely none as ammonia.

In dealing with urea there is no difficulty in making it take up the elements of water and evolve all its nitrogen in the form of ammonia. This may be very conveniently accomplished by maintaining it for a short time at a temperature of  $150^{\circ}$  C. in contact with caustic potash. Now this peculiar character—to yield up nitrogen in the form of ammonia on heating to  $150^{\circ}$  C. with caustic alkali, and not to yield up nitrogen as ammonia when oxidized—appears to pertain exclusively to urea. Not only does *free* urea exhibit this character, but *coupled* urea shows it also; thus creatine, which is urea coupled with sarcosine, yields only the ammonia arising from the sarcosine when it is oxidized by alkaline permanganate, yielding the ammonia from its urea only on treatment with alkali.

Reasoning from this behaviour of coupled urea, I have been led to the interesting conclusion that albumen, caseine, and fibrine are coupled ureas, but that gelatine is not a coupled urea.

Although all nitrogenous organic compounds, with the exception of nitro-compounds, ferrocyanides, and urea, yield ammonia to alkaline permanganate, yet many nitrogenous organic compounds do not yield their total nitrogen in the form of ammonia. The natural alkaloids, as for example morphia, often give up half of the nitrogen as ammonia, as is also the case with naphthylamine and toluidine.

From 100 grms. of albuminous substances about 10 grms. are obtained by means of alkaline permanganate.

When my colleagues and I first directed our attention to the subject of water, we endeavoured to provide a test which should not fail to recognize germs by their chemical characters—a test which could not fail to distinguish between water that was pure and water that was charged with germs. With this object in view, we selected egg-albumen as the representative of germs, and then sought for the most accurate and convenient method of measuring the strength of excessively dilute solutions of it.

How accurately the ammonia process effects this object may be judged of by the following examples, which I quote from a paper of mine published by the Chemical Society in the year 1867.

Moist white of egg. milligrammes.	Ammonia found. milligrammes.	Ammonia calculated. milligrammes.
i. 17.69	0.210	0.214
ii. 17.58	0.213	0.2127
iii. 41.80	0.505	0.5058
iv. 27.87	0.350	0.337
v. 12.20	0.145	0.1476
vi. 7.47	0.095	0.0904
vii. 23.065	0.275	0.279

The "moist white of egg" used in these experiments was prepared and divided in this way:—The egg-shell having been broken and the white separated from the yelk by mechanical means, a quantity of the white (without any drying) was weighed out and dissolved in water to which a little carbonate of soda had been added. To the solution sufficient water was added until the weight of the whole solution was equal to one hundred times the weight of the white of egg originally taken. In this manner a solution, whereof one part by weight contained one hundredth of white of egg, was prepared. The weighed quantity of the solution was placed in a retort containing 400 cubic centimetres of carefully purified water, and submitted to the action of the permanganate.

In the above experiments it will be observed that the smallest quantity operated upon was 7.47 milligrammes, equal to about 1 milligramme of dry albumen. The largest quantity was about 6 milligrammes of dry albumen.

I have no hesitation in saying that by the ammonia process there is no difficulty in measuring 1 milligramme of dry albumen in a litre of water with considerable accuracy; and there is certainly no difficulty in detecting  $\frac{1}{20}$  milligramme of dry albumen in a litre of water.

With what substances does the ammonia process confound albumen? With every description of organic nitrogenous substance that water contains; with the exception of urea.

We have named the ammonia generated by the action of permanganate "albuminoid ammonia," adopting a technical term based upon the history of our research, and we hope that the name may be preserved.

One of the points to which attention has not been called in the various controversies which have arisen, is the fact that there is no danger of nitrates being confounded with albumen by the ammonia process. This is of more importance than might seem at first sight, inasmuch as it is no uncommon thing for a sample of natural water to contain ten or twenty times as much nitrates as organic nitrogenous matter. The consideration that the process by which the ammonia is obtained is an oxidizing process—



backed up, as it is, by the direct experiment on the action of alkaline permanganate on picric acid, in which there was no production of ammonia, and by the general observation that samples of water sometimes contain very little organic nitrogenous matter and very much nitrates—is decisive against there being any risk of confounding nitrates with albumen.

In making a *résumé* of the main results of an extended application of the new method of analysis, I have to note the fact of the extraordinary purity of natural water. Instead of about a grain per gallon of organic nitrogenous matter (as indeed seemed not unlikely from the older results obtained by incinerating water-residues), the usual proportion of nitrogenous organic matter does not amount to so much as  $\frac{1}{10}$  grain per gallon, the “albuminoid ammonia” being some 0·06 or 0·07 parts per million. By filtration, either through a filter of sand and charcoal or naturally through porous strata, water attains to exquisite purity.

The following results illustrate these facts. In one litre :—

	Milligrammes of albuminoid ammonia.
New River, London . . . . .	0·05
Thames, West Middlesex Co., London	0·06
Woodhead water, Manchester . . .	0·07
Edinburgh water-supply . . . . .	0·07
Glasgow, from Loch Katrine . . . .	0·08
Chester, from Dee . . . . .	0·06
Scarborough . . . . .	0·06
Spring, from Greensand . . . . .	0·00
Caterham, Kent . . . . .	0·00
Kent Water Co. . . . .	0·02
Somersetshire . . . . .	0·01
Guildford, Surrey . . . . .	0·01
Carefully filtered water . . . . .	0·01

## LXVII. *Proceedings of Learned Societies.*

### ROYAL SOCIETY.

[Continued from p. 473.]

Feb. 15, 1872.—George Biddell Airy, C.B., President, succeeded by Mr. C. B. Vignoles (as Deputy appointed by the President), in the Chair.

THE following communication was read :—

“On the Induction of Electric Currents in an Infinite Plane Sheet of uniform conductivity.” By Prof. J. Clerk Maxwell, F.R.S.

1. When, on account of the motion or the change of strength

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2 M

of any magnet or electromagnet, a change takes place in the magnetic field, electromotive forces are called into play; and if the material in which they act is a conductor, electric currents are produced. This is the phenomenon of the induction of electric currents, discovered by Faraday.

I propose to investigate the case in which the conducting substance is in the form of a thin stratum or sheet, bounded by parallel planes, and of indefinite extent. A system of magnets or electromagnets is supposed to exist on the positive side of this sheet, and to vary in any way by changing its position or its intensity. We have to determine the nature of the currents induced in the sheet, and their magnetic effect at any point, and in particular their reaction on the electromagnetic system which gave rise to them. The induced currents are due, partly to the direct action of the external system, and partly to their mutual inductive action; so that the problem appears, at first sight, somewhat difficult.

2. The result of the investigation, however, may be presented in a remarkably simple form, by the aid of the principle of images, which was first applied to problems in electricity and hydrokinetics by Sir W. Thomson. The essential part of this principle is, that we conceive the state of things on the positive side of a certain closed or infinite surface (which is really caused by actions having their seat on that surface) to be due to an imaginary system on the negative side of the surface, which, if it existed, and if the action of the surface were abolished, would give rise to the actual state of things in the space on the positive side of the surface.

The state of things on the positive side of the surface is expressed by a mathematical function, which is different in form from that which expresses the state of things on the negative side, but which is identical with that which would be due to the existence, on the negative side, of a certain system which is called the Image.

The image, therefore, is what we should arrive at by *producing*, as it were, the mathematical function as far as it will go—just as, in optics, the virtual image is found by producing the rays, in straight lines, backwards from the place where their direction has been altered by reflexion or refraction.

3. The position of the image of a point in a plane surface is found by drawing a perpendicular from the point to the surface and producing it to an equal distance on the other side of the surface. If the image is of the same sign as the point, as it is in hydrokinetics when the surface is a rigid plane, it is called a positive image. If it is of the opposite sign, as in statical electricity, when the surface is a conductor, it is called a negative image. The image of a conducting circuit is reckoned positive when the electric current flows in the corresponding directions through corresponding parts of the object and the image. The image is reckoned negative when the direction of the current is reversed.

In the case of the plane conducting sheet, the imaginary system on the negative side of the sheet is not the simple image, positive or negative, of the real magnet or electromagnet on the positive

side, but consists of a moving train of images, the nature of which we now proceed to define.

4. Let the electric resistance of a rectangular portion of the sheet whose length is  $a$ , and whose breadth is  $2\pi a$ , be  $R$ .

$R$  is to be measured on the electromagnetic system, and is therefore a velocity, the value of which is independent of the magnitude of the line  $a$ . (If  $\rho$  denotes the specific resistance of the material of the sheet for a unit cube, and if  $c$  is the thickness of the sheet, then

$$R = \frac{\rho}{2\pi c}; \text{ and if } \sigma \text{ denotes the specific resistance of the sheet for}$$

a unit (or any other) square,  $R = \frac{\sigma}{2\pi}$ .)

5. Let us begin by dividing the time into a number of equal intervals, each equal to  $\delta t$ . The smaller we take these intervals the more accurate will be the definition of the train of images which we shall now describe.

6. At a given time  $t$ , let a positive image of the magnet or electromagnet be formed on the negative side of the sheet.

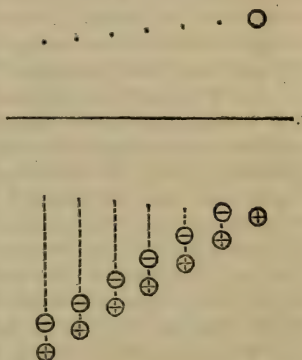
As soon as it is formed, let this image begin to move away from the sheet in the direction of the normal, with the velocity  $R$ , its form and intensity remaining constantly the same as that which the magnet had at the time  $t$ .

After an interval  $\delta t$  (that is to say, at the time  $t + \delta t$ ) let a negative image, equal in magnitude and opposite in sign to this positive image, be formed in the original position of the positive image, and let it then begin to move along the normal, after the positive image, with the velocity  $R$ . The interval of time between the arrival of these images at any point will be  $\delta t$ , and the distance between corresponding points will be  $R\delta t$ .

7. Leaving this pair of images to pursue their endless journey, let us attend to the real magnet, or electromagnet, as it is at the time  $t + \delta t$ . At this instant let a new positive image be formed of the magnet in its new position, and let this image also travel in the direction of the normal with the velocity  $R$ , and be followed after an interval of time  $\delta t$  by a corresponding negative image. Let these operations be repeated at equal intervals of time, each of these intervals being equal to  $\delta t$ .

8. Thus at any given instant there will be a train or trail of images, beginning with a single positive image, and followed by an endless succession of pairs of images. This trail, when once formed, continues unchangeable in form and intensity, and moves as a whole away from the conducting sheet with the constant velocity  $R$ .

9. If we now suppose the interval of time  $\delta t$  to be diminished without limit, and the train to be extended without limit in the ne-





gative direction, so as to include all the images which have been formed in all past time, the magnetic effect of this imaginary train at any point on the positive side of the conducting sheet will be identical with that of the electric currents which actually exist in the sheet.

Before proceeding to prove this statement, let us take notice of the form which it assumes in certain cases.

10. Let us suppose the real system to be an electromagnet, and that its intensity, originally zero, suddenly becomes  $I$ , and then remains constant. At this instant a positive image is formed, which begins to travel along the normal with velocity  $R$ . After an interval  $\delta t$  another positive image is formed; but at the same instant a second negative image is formed at the same place, which exactly neutralizes its effect. Hence the result is, that a single positive image travels by itself along the normal with velocity  $R$ . The magnetic effect of this image on the positive side of the sheet is equivalent to that of the currents of induction actually existing in the sheet; and the diminution of this effect, as the image moves away from the sheet, accurately represents the effect of the currents of induction, which gradually decay on account of the resistance of the sheet. After a sufficient time, the image is so distant that its effects are no longer sensible on the positive side of the sheet. If the current of the electromagnet be now broken, there will be no more images; but the last negative image of the train will be left unneutralized, and will move away from the sheet with velocity  $R$ . The currents in the sheet will therefore be of the same magnitude as those which followed the excitement of the electromagnet, but in the opposite direction.

11. It appears from this that, when the electromagnet is increasing in intensity, it will be acted on by a repulsive force from the sheet; and when its intensity is diminishing, it will be attracted towards the sheet.

It also appears that if any system of currents be produced in the sheet and then left to itself, the effect of the decay of the currents, as observed at a point on the positive side of the sheet, will be the same as if the sheet, with its currents remaining constant, had been carried away in the negative direction with velocity  $R$ .

12. If a magnetic pole of strength  $m$  be brought from an infinite distance along a normal to the sheet with a uniform velocity  $v$  towards the sheet, it will be repelled with a force

$$\frac{m^2}{4\pi^2} \frac{v}{R+v},$$

where  $z$  is the distance from the sheet at the given instant.

This formula will not apply to the case of the pole moving away from the sheet, because in that case we must take account of the currents which are excited when the pole begins to move, which it does when near the sheet.

13. If the magnetic pole move in a straight line parallel to the sheet, with uniform velocity  $v$ , it will be acted on by a force in the

opposite direction to its motion, and equal to

$$\frac{m^2}{4z^2} v \frac{\sqrt{R^2 + v^2} + R - v}{(\sqrt{R^2 + v^2} + R)^2}.$$

Besides this retarding force, it is acted on by a force repelling it from the sheet, equal to

$$\frac{m^2}{4z^2} \frac{v^2}{R^2 + v^2 + R \sqrt{R^2 + v^2}}.$$

14. If the pole moves uniformly in a circle, the trail is in the form of a helix, and the calculation of its effect is more difficult; it is easy, however, to see that, besides the retarding force and the repelling force, there is also a force towards the centre of the circle.

15. It is shown, in my treatise on Electricity and Magnetism (vol. ii. art. 600), that the currents in any system are the same, whether the conducting system or the inducing system be in motion, provided the relative motion is the same. Hence the results already given are directly applicable to the case of Arago's rotating disk, provided the induced currents are not sensibly affected by the limitation arising from the edge of the disk. These will introduce other sets of images, which we shall not now investigate.

16. The greater the resistance of the sheet, whether from its thinness or from the low conducting-power of its material, the greater is the velocity  $R$ . Hence in most actual cases  $R$  is very great compared with  $v$ , the velocity of the external system, and the trail of images is nearly normal to the sheet, and the induced currents differ little from those which arise from the direct action of the external system (see § 1).

17. If the conductivity of the sheet were infinite, or its resistance zero,  $R$  would be zero. The images, once formed, would remain stationary, and all except the last formed positive image would be neutralized. Hence the trail would be reduced to a single positive image, and the sheet would exert a repulsive force  $\frac{m^2}{4z^2}$  on the pole, whether the pole were in motion or at rest.

I need not say that this case does not occur in nature as we know it. Something of the kind is supposed to exist in the interior of molecules in Weber's Theory of Diamagnetism.

#### *Mathematical Investigation.*

18. Let the conducting sheet coincide with the plane of  $xy$ , and let its thickness be so small that we may neglect the variation of magnetic force at different points of the same normal within its substance, and that, for the same reason, the only currents which can produce sensible effects are those which are parallel to the surface of the sheet.

#### *Current-function.*

19. We shall define the currents in the sheet by means of the current-function  $\phi$ . This function expresses the quantity of electri-

city which, in unit of time, crosses from right to left a curve drawn from a point at infinity to the point P.

This quantity will be the same for any two curves drawn from this point to P, provided no electricity enters or leaves the sheet at any point between these curves. Hence  $\phi$  is a single-valued function of the position of the point P.

The quantity which crosses the element  $ds$  of any curve from right to left is

$$\frac{d\phi}{ds} ds.$$

By drawing  $ds$  first perpendicular to the axis of  $x$ , and then perpendicular to the axis of  $y$ , we obtain for the components of the electric current in the directions of  $x$  and of  $y$  respectively

$$u = \frac{d\phi}{dy}, \quad v = -\frac{d\phi}{dx}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

The curves for which  $\phi$  is constant are called current lines.

20. The annular portion of the sheet included between the current lines  $\phi$  and  $\phi + \delta\phi$  is a conducting circuit round which an electric current of strength  $\delta\phi$  is flowing in the positive direction—that is, from  $x$  towards  $y$ . Such a circuit is equivalent in its magnetic effects to a magnetic shell of strength  $\delta\phi$ , having the circuit for its edge\*.

The whole system of electric currents in the sheet will therefore be equivalent to a complex magnetic shell, consisting of all the simple shells, defined as above, into which it can be divided. The strength of the equivalent complex shell at any point will be  $\phi$ .

We may suppose this shell to consist of two parallel plane sheets of imaginary magnetic matter at a very small distance  $c$ , the surface-density being  $\frac{\phi}{c}$  on the positive sheet, and  $-\frac{\phi}{c}$  on the negative sheet.

21. To find the magnetic potential due to this complex plane shell at any point not in its substance, let us begin by finding P, the potential at the point  $(\xi, \eta, \zeta)$  due to a plane sheet of imaginary magnetic matter whose surface-density is  $\phi$ , and which coincides with the plane of  $xy$ . The potential due to the positive sheet whose surface-density is  $\frac{\phi}{c}$ , and which is at a distance  $\frac{1}{2}c$  on the positive side of the plane of  $xy$ , is

$$\frac{1}{c} \left( P - \frac{1}{2}c \frac{dP}{d\zeta} + \&c. \right).$$

That due to the negative sheet, at a distance  $\frac{1}{2}c$  on the negative side of the plane of  $xy$ , is

$$-\frac{1}{c} \left( P + \frac{1}{2}c \frac{dP}{d\zeta} + \&c. \right).$$

Hence the magnetic potential of the shell is

$$V = -\frac{dP}{d\zeta}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

\* W. Thomson, "Mathematical Theory of Magnetism," Phil. Trans. 1850.



This, therefore, is the value of the magnetic potential of the current-sheet at any given point on the positive side of it. Within the sheet there is no magnetic potential, and at any point  $(\xi, \eta, -\zeta)$  on the negative side of the sheet the potential is equal and of opposite sign to that at the point  $(\xi, \eta, \zeta)$  on the positive side.

22. At the positive surface the magnetic potential is

$$V = -\frac{dP}{d\zeta} = 2\pi\phi. \quad (3)$$

At the negative surface

$$\frac{dP}{d\xi} = 2\pi\phi. \quad (4)$$

The normal component of magnetic force at the positive surface is

$$\gamma = -\frac{dV}{d\zeta} = \frac{d^2P}{d\zeta^2}. \quad (5)$$

In the case of the magnetic shell, the magnetic force is discontinuous at the surface; but in the case of the current-sheet this expression gives the value of  $\gamma$  within the sheet itself, as well as in the space outside.

23. Let  $F, G, H$  be the components of the electromagnetic momentum at any point in the sheet, due to external electromagnetic action as well as to that of the currents in the sheet, then the electromotive force in the directions of  $x$  is

$$-\frac{dF}{dt} - \frac{d\psi}{dx},$$

where  $\psi$  is the electric potential\*; and by Ohm's law this is equal to  $\sigma u$ , where  $\sigma$  is the specific resistance of the sheet.

Hence

$$\left. \begin{aligned} \sigma u &= -\frac{dF}{dt} - \frac{d\psi}{dx} \\ \sigma v &= -\frac{dG}{dt} - \frac{d\psi}{dy} \end{aligned} \right\} \quad (6)$$

Similarly;

Let the external system be such that its magnetic potential is represented by  $-\frac{dP_0}{dz}$ , then the actual magnetic potential will be

$$V = -\frac{d}{dz}(P_0 + P), \quad (7)$$

and

$$F = \frac{d}{dy}(P_0 + P), \quad G = -\frac{d}{dx}(P_0 + P), \quad H = 0. \quad (8)$$

Hence equations (6) become, by introducing the stream-function

\* "Dynamical Theory of the Electromagnetic Field," Phil. Trans. 1865, p. 483.

$\phi$  from (1),

$$\left. \begin{aligned} \sigma \frac{d\phi}{dy} &= -\frac{d^2}{dt dy} (P_0 + P) - \frac{d\psi}{dx}, \\ -\sigma \frac{d\phi}{dx} &= \frac{d^2}{dt dx} (P_0 + P) - \frac{d\psi}{dy}. \end{aligned} \right\} \dots \dots (9)$$

A solution of these equations is

$$\sigma\phi = -\frac{d}{dt} (P_0 + P), \quad \psi = \text{constant.} \dots \dots (10)$$

Substituting the value of  $\phi$  in terms of  $P$ , as given in equation (4),

$$\frac{\sigma}{2\pi} \frac{dP}{dz} = \frac{d}{dt} (P_0 + P). \dots \dots (11)$$

The quantity  $\frac{\sigma}{2\pi}$  is evidently a velocity; let us therefore for conciseness call it  $R$ , then

$$\frac{dP}{dz} + \frac{dP}{dt} + \frac{dP_0}{dt} = 0. \dots \dots (12)$$

24. Let  $P_0'$  be the value of  $P_0$  at the time  $t-\tau$ , and at a point on the negative side of the sheet, whose coordinates are  $x, y, (z-R\tau)$ , and let

$$Q = \int_0^\infty P_0' d\tau. \dots \dots (13)$$

At the upper limit when  $\tau$  is infinite  $P_0'$  vanishes. Hence at the lower limit, when  $\tau=0$  and  $P_0'=P_0$ , we must have

$$P_0 = \frac{dQ}{dt} + R \frac{dQ}{dz}; \dots \dots (14)$$

but by equation (12)

$$\frac{dP_0}{dt} = -\frac{dP}{dt} - R \frac{dP}{dz}. \dots \dots (15)$$

Hence the equation will be satisfied if we make

$$P = -\frac{dQ}{dt} = -\frac{d}{dt} \int_0^\infty P_0' d\tau. \dots \dots (16)$$

25. This, then, is a solution of the problem. Any other solution must differ from this by a system of closed currents, depending on the initial state of the sheet, not due to any external cause, and which therefore must decay rapidly. Hence, since we assume an eternity of past time, this is the only solution of the problem.

This solution expresses  $P$ , a function due to the action of the induced current, in terms of  $P_0'$ , and through this of  $P_0$ , a function of the same kind due to the external magnetic system. By differentiating  $P$  and  $P_0$  with respect to  $z$ , we obtain the magnetic potential, and by differentiating them with respect to  $t$ , we obtain, by equation (10), the current-function. Hence the relation between  $P$  and  $P_0$ , as expressed by equation (16), is similar to the relation

between the external system and its trail of images as expressed in the description of these images in the first part of this paper (§§ 6, 7, 8, 9), which is simply an explanation of the meaning of equation (16) combined with the definition of  $P_0'$  in § 24.

#### NOTE TO THE PRECEDING PAPER.

At the time when this paper was written, I was not able to refer to two papers by Prof. Felici, in Tortolini's '*Annali di Scienze*' for 1853 and 1854, in which he discusses the induction of currents in solid homogeneous conductors and in a plane conducting sheet, and to two papers by E. Jochmann in Crelle's *Journal* for 1864, and one in Poggendorff's '*Annalen*' for 1864, on the currents induced in a rotating conductor by a magnet.

Neither of these writers has attempted to take into account the inductive action of the currents on each other, though both have recognized the existence of such an action, and given equations expressing it. M. Felici considers the case of a magnetic pole placed almost in contact with a rotating disk. E. Jochmann solves the case in which the pole is at a finite distance from the plane of the disk. He has also drawn the forms of the current-lines and of the equipotential lines, in the case of a single pole, and in the case of two poles of opposite name at equal distances from the axis of the disk, but on opposite sides of it, and has pointed out why the current-lines are not, as Matteucci at first supposed, perpendicular to the equipotential lines, which he traced experimentally.

I am not aware that the principle of images, as described in the paper presented to the Royal Society, has been previously applied to the phenomena of induced currents, or that the problem of the induction of currents in an infinite plane sheet has been solved, taking into account the mutual induction of these currents, so as to make the solution applicable to a sheet of any degree of conductivity.

The statement in equation (10), that the motion of a magnetic system does not produce differences of potential in the infinite sheet, may appear somewhat strange, since we know that currents may be collected by electrodes touching the sheet at different points. These currents, however, depend entirely on the inductive action on the part of the circuit not included in the sheet; for if the whole circuit lies in the plane of the sheet, but is so arranged as not to interfere with the uniform conductivity of the sheet, there will be no difference of potential in any part of the circuit. This is pointed out by Felici, who shows that when the currents are induced by the instantaneous magnetization of a magnet, these currents are not accompanied by differences of potential in different parts of the sheet.

When the sheet is itself in motion, it appears, from art. 600 of my treatise '*On Electricity and Magnetism*,' that the electric potential of any point, as measured by means of the electrodes of a fixed circuit, is

$$\psi = - \left( F \frac{\partial x}{\partial t} + G \frac{\partial y}{\partial t} + H \frac{\partial z}{\partial t} \right),$$



where  $\frac{\partial x}{\partial t}$ ,  $\frac{\partial y}{\partial t}$ ,  $\frac{\partial z}{\partial t}$  are the components of the velocity of the part of the sheet to which the electrode is applied.

In the case of a sheet revolving with velocity  $\omega$  about the axis of  $z$ , this becomes

$$\psi = \omega \left( x \frac{dP}{dx} + y \frac{dP}{dy} \right).$$

*Note 2.*—The velocity  $R$  for a copper plate of best quality 1 millimetre in thickness is about 25 metres per second. Hence it is only for *very* small velocities of the apparatus that we can obtain any approximation to the true result by neglecting the mutual induction of the currents.—Feb. 13.

Feb. 22.—William Spottiswoode, M.A., Treasurer and Vice-President, in the Chair.

The following communication was read :—

“On a New Hygrometer.” By Wildman Whitehouse, Esq.

The use of Mason’s wet-bulb thermometer as a means of hygrometric measurement, though admitted to be the most practically useful, and indeed the only recording instrument for the purpose, has yet this serious inconvenience, not to say defect, viz. that its indications either cease or are valueless at temperatures below 32° F.

In a conversation which the writer had with the Director of the Meteorological Office some months ago, the question arose whether any thing could be suggested to remedy this inconvenience.

It was obviously inadmissible to substitute any other fluid for, or to make any addition to, the water employed for the wet bulb, as then it would cease to be a test for the purely hygrometric capacity of the air. It became therefore necessary to fall back in another direction, and to find some hygrometric body which should readily and rapidly absorb moisture from the air, and at the same time afford some means of measuring and recording the amount of such absorption.

Fused chloride of zinc or of calcium seemed promising, as very active agents, absorbing rapidly on their surface, and allowing the readiest possible escape of the fluid hydrate for measurement; yet no means presented itself either of accurately measuring, regulating, or maintaining the exact extent of surface exposed for absorption; nor could the substance itself be easily renewed when required; nor, indeed, could either of these substances be regarded as wholly free from the interference of frost, as the moisture absorbed from the atmosphere at a temperature much below freezing-point may remain frozen on the surface, and become incapable of continuous measurement. It seemed essential to the accuracy and practical utility of any instrument designed for this purpose :—

1st. That a fixed and invariable extent of surface should at all times be exposed for absorption of moisture ;

2nd. That the apparatus should be simple, inexpensive, and not inconvenient in use ;

3rd. That the hygrometric substance should be continuously and steadily renewable ; and above all, if it were possible,

4th. That the measurement should be effected thermometrically.

No solid hygrometric substance seemed capable of meeting these requirements ; but all the conditions seemed likely to be fulfilled by the use of concentrated sulphuric acid. This would admit of being spread in an exquisitely fine film over the surface of the bulb of a thermometer by means of a glass capillary siphon, of which one end should rest on the upper part of the bulb, while the other end dipped into a reservoir of the acid. A continuous supply could be maintained for any required length of time by suitable arrangements. The absorption of moisture would necessarily be attended by a rise in temperature, and this would be proportioned to the amount of hygrometric moisture absorbed ; while the hydrated acid, having fulfilled its office, would fall in drops from the bulb into any tube or reservoir placed for the purpose.

An instrument has been constructed by the writer to test this principle, which has, by the courtesy of the Director of the Meteorological Office, been under observation for some weeks.

It consists essentially of three thermometers of similar construction, and used as a "wet bulb," a "dry bulb," and an "acid bulb," respectively, placed side by side on a suitable frame, and read together for comparison.

The experience already gained in the use of this instrument has shown that, with a reservoir of proper construction, the supply of acid may be made continuous for any required length of time, and that, from the very slight variations of flow which occur in its action, the supply to the thermometer will be sensibly equable.

The length of the siphon, and the size of the capillary bore, together with the difference of level between the surface of the fluid in the trough of the reservoir and the point of delivery on the bulb, will determine the rate of supply of the acid.

It is clear that either a too rapid and continuous stream of acid at the temperature of the air, or a too scanty supply, would diminish the readings ; yet it is found that practically there may be a pretty wide range of variation in the supply of acid, within which no essential change in the sensibility of the instrument is noticed.

For a bulb having one square inch of surface one drop per minute is sufficient, though the time may range from 40 to 100 seconds without inconvenience, the time being noted as the hydrated acid, after having fulfilled its office, falls drop by drop from the bulb.

The quantity of acid required at this rate is about 3 fluid ozs. per diem, or one imperial pint per week, which is procurable of uniform density, sufficiently pure and free from lead, at a cost of about  $2\frac{1}{2}d$ .

The temperature of the acid in the reservoir is of course that of the surrounding air ; the elevation of temperature shown by the acid-bulb thermometer is due to, and seems to be strictly a measure of, the amount of moisture absorbed by the film of acid spread on

the surface of the bulb, say one square inch, continuously supplied in its concentrated state, and as constantly passing off hydrated.

While, therefore, this instrument is, like Mason's, intended to measure the amount of hygrometric moisture in the air, and to do so thermometrically, it yet is, in its principle and in its operation, essentially of an opposite character.

The ordinary wet-bulb thermometer is at the zero of its scale in an atmosphere of perfect saturation, and its action depends upon the amount of sensible heat absorbed and rendered latent by evaporation of the water from its surface.

The acid-bulb thermometer is at its zero in a perfectly dry atmosphere; and its action depends upon the amount of latent heat rendered sensible by the condensation of vapour into water on the surface of the bulb, and by the combination of this water with the concentrated acid.

It would appear that an hygrometer on this principle is entirely free from the action of frost; while its sensibility is so great as to be at first almost embarrassing.

This may, however, be easily regulated and toned down, if necessary, to any required range by the dilution of the acid with glycerine, a fluid which is also of itself hygrometric, though its thermal effects are far less marked than those of sulphuric acid.

The following series of observations, made hourly and otherwise, at intervals during the past few weeks, at the Meteorological Office, by the kindness of the Director, will suffice to show approximately the relations of the "acid" and the "wet bulb" respectively.

They have been chiefly actual out-door observations, and have extended over a considerable range of temperature and atmospheric variations.

It will require a most careful series of observations to elicit all the points noteworthy in the new instrument, and to determine the relative values of the wet- and acid-bulb readings, noting the behaviour of each at every part of the scale, from absolute dryness to saturation, and at temperatures ranging from  $75^{\circ}$  or  $80^{\circ}$  down to  $0^{\circ}$ .

This will be necessary before the instrument can aspire to take its place among the recognized standards of meteorological science; but in the mean time the writer has been advised to offer, at the earliest time, a brief description of it to the notice of the Royal Society.



TABLE.—Giving comparison of Readings of Wet- and Acid-bulb Hygrometers.

1871.		Mason's Hygrometer.			Mr. Whitehouse's.		Deductions from Wet-bulb Hygrometer.			Remarks.
Day.	Hour.	Dry Bulb.	Wet Bulb.	Difference, D-W.	Acid Bulb.	Difference, A-D.	Tension of Vapour.	Weight of Vapour in 1 cub. foot of Air.	Relative Humidity, Saturation = 100.	
	Acid flowing from bulb 1 drop in 33 seconds.									
	h m	°	°	°	°	°	in.	grs.	ds.	
Nov. 6	10 7 A.M.	40°0	36°0	4°0	60°2	20°2	·172	2°0	69	
"	10 30 "	40°5	36°3	4°2	61°5	21°0	·173	2°0	69	
"	11 25 "	41°0	36°7	4°3	61°3	20°3	·175	2°0	68	
"	11 33 "	41°0	37°0	4°0	62°0	21°0	·180	2°1	70	{ At noon the wet bulb was washed and resupplied with water.
"	1 4 P.M.	43°0	38°0	5°0	64°0	21°0	·181	2°1	65	
"	1 50 "	44°0	38°5	5°5	65°2	21°2	·180	2°1	62	
"	2 5 "	44°0	39°0	5°0	65°3	21°3	·188	2°2	65	{ Sky has gradually become overcast since the morning.
"	2 20 "	43°7	38°8	4°9	65°9	22°2	·188	2°2	66	
"	2 30 "	44°0	39°0	5°0	65°7	21°7	·188	2°2	65	
"	3 30 "	44°0	39°3	4°7	66°3	22°3	·193	2°3	67	
"	3 55 "	44°0	39°6	4°4	67°0	23°0	·198	2°3	68	
Nov. 7	9 45 A.M.	45°0	43°0	2°0	75°0	30°0	·253	2°9	85	{ Note the change in night! Dark and foggy day.
"	10 10 "	45°5	44°0	1°5	77°8	32°3	·270	3°1	89	
"	10 20 "	46°0	44°2	1°8	76°0	30°0	·269	3°1	87	(!)
"	10 30 "	46°0	44°2	1°8	76°2	30°2	·269	3°1	87	
"	11 5 "	46°8	45°0	1°8	75°2	28°4	·277	3°1	87	(!)
"	11 35 "	47°7	45°6	2°1	78°0	30°3	·276	3°2	87	
"	1 15 P.M.	50°0	47°0	3°0	82°0	32°0	·286	3°3	80	{ Vapour has increased, but humidity has decreased.
"	3 15 "	50°0	47°0	3°0	82°4	32°4	·286	3°3	80	
"	3 50 "	50°0	47°0	3°0	82°9	32°9	·286	3°3	80	
"	4 0 "	50°0	47°0	3°0	83°0	33°0	·286	3°3	80	
"	5 0 "	49°5	46°5	3°0	83°5	34°0	·281	3°2	80	
"	6 0 "	49°5	46°5	3°0	82°5	33°0	·281	3°2	80	
"	8 0 "	51°0	47°0	4°0	81°5	30°5	·276	3°1	74	
"	9 0 "	49°5	46°0	3°5	79°5	30°0	·270	3°0	76	
Nov. 8	10 40 A.M.	47°5	45°5	2°0	81°8	34°3	·280	3°3	86	{ Sky overcast; slight misty drizzle. Vapour increased; humidity decr.
"	10 50 "	50°3	48°0	2°3	89°0	38°7	·305	3°5	84	
"	11 15 "	50°0	47°6	2°4	88°0	38°0	·300	3°4	83	
"	11 25 "	50°0	47°0	3°0	85°3	35°3	·286	3°3	80	{ Sky clearing since 11.25 A.M. Humidity greatly reduced; vapour not so much. Vapour decreased in greater proportion than humidity.
"	NOON	50°3	47°0	3°3	82°0	31°7	·283	3°3	78	
"	1 50 P.M.	51°0	45°5	5°5	81°0	30°0	·246	2°8	63	
"	2 30 "	49°7	43°0	6°7	75°2	25°5	·210	2°4	59	
"	3 0 "	49°0	43°2	5°8	76°0	27°0	·221	2°5	63	
"	3 50 "	48°7	43°0	5°7	74°7	26°0	·219	2°5	66	

TABLE (continued).

1871.		Mason's Hygrometer.			Mr. White-house's.		Deductions from Wet-bulb Hygrometer.			Remarks.
Day.	Hour.	Dry Bulb.	Wet Bulb.	Difference, D-W.	Acid Bulb.	Difference, A-D.	Tension of Vapour.	Weight of Vapour in 1 cub. foot of Air.	Relative Humidity. Saturation = 100.	
Nov. 9 ..	h m 9 0 A.M.	37°0	35°0	2°0	58°5	21°5	182	2.1	83	Vapour much decreased; yet humidity has risen. Note acid-bulb.
" ..	9 10 "	37°0	35°0	2°0	58°0	21°0	182	2.1	83	
" ..	0 20 P.M.	45°0	40°0	5°0	68°0	23°0	197	2.3	66	
" ..	2 0 "	45°0	39°2	5°8	66°0	21°0	184	2.1	61	Fine day; rather cloudy P.M.
" ..	3 0 "	45°0	39°0	6°0	66°7	21°7	181	2.1	60	
" ..	3 30 "	45°0	39°2	5°8	66°0	21°0	184	2.1	61	
" ..	3 45 "	44°5	39°0	5°5	65°7	21°2	185	2.1	63	Vapour slightly increased, but humidity decreased. Note acid-bulb.
" ..	6 0 "	42°0	38°5	3°5	63°5	21°5	198	2°3	76	
" ..	6 30 "	42°0	38°5	3°5	62°5	20°5	198	2°3	76	
" ..	7 0 "	41°0	38°0	3°0	62°0	21°0	197	2°3	77	Slight shower at 4.10 P.M.
" ..	8 0 "	39°5	37°0	2°5	63°0	23°5	194	2°3	80	
" ..	9 0 "	39°5	36°5	3°0	61°0	21°5	185	2°1	77	
Nov. 10..	11 30 A.M.	43°0	39°0	4°0	67°0	24°0	181	2°1	65	Note these changes.
" ..	11 40 "	42°5	39°0	3°5	67°2	24°7	202	2°3	75	
" ..	3 10 P.M.	45°0	39°0	6°0	68°0	23°0	181	2°1	60	
" ..	6 0 "	42°5	37°0	5°5	62°0	19°5	170	1°9	63	
" ..	6 30 "	41°5	37°0	4°5	61°0	19°5	177	2°0	67	
" ..	7 30 "	40°0	36°5	3°5	60°0	20°0	180	2°1	73	
" ..	8 0 "	39°5	36°0	3°5	61°0	21°5	176	2°1	74	
" ..	8 30 "	38°5	35°5	3°0	60°0	21°5	177	2°1	76	
" ..	9 0 "	38°5	35°0	3°5	59°5	21°0	169	2°0	80	Foggy and cold.
Nov. 11..	8 50 A.M.	33°0	32°0	1°0	52°5	19°5	167	2°0	89	
" ..	9 50 "	35°0	33°0	2°0	55°0	20°0	164	1°9	80	
" ..	11 20 "	39°0	36°5	2°5	62°0	23°0	190	2°2	80	Vapour increased; humidity steady.
Nov. 4 ..	Acid flowing about 1 drop in 77 seconds. 2 50 P.M.	48°0	44°5	3°5	71°5	23°5	253	2°9	76	Acid-reading doubtful; taken too soon after starting.
" ..	3 10 "	47°3	44°0	3°3	74°3	27°0	258	2°9	77	
" ..	3 30 "	47°5	44°3	3°2	75°0	27°5	255	2°85	78	
" ..	3 50 "	47°2	44°0	3°2	74°0	26°8	251	2°9	78	
Nov. 24..	11 24 A.M.	44°0	41°2	2°8	66°5	22°5	228	2°6	78	Humidity unaltered; vapour decreased.
" ..	5 30 P.M.	42°0	38°5	3°5	63°3	21°3	199	2°3	75	
" ..	6 0 "	41°5	38°0	3°5	63°0	21°5	193	2°3	75	
" ..	6 30 "	41°5	37°5	4°0	63°0	21°5	185	2°2	71	Vapour hardly changed; humidity very largely increased. Note acid
Nov. 25..	11 30 A.M.	38°5	36°8	1°7	61°0	22°5	200	2°3	86	
Nov. 29..	11 15 "	39°0	37°0	2°0	61°5	22°5	199	2°3	84	
" ..	11 25 "	39°0	37°0	2°0	61°0	22°0				
" ..	3 45 P.M.	40°0	37°3	2°7	62°5	22°5				
" ..	5 0 "	39°0	37°5	1°5	61°0	22°0				Raining.
" ..	6 0 "	38°0	36°0	2°0	59°0	21°0				
" ..	6 30 "	38°0	35°5	2°5	58°5	20°0				
Nov. 30..	2 0 "	39°0	37°0	2°0	62°0	23°0				
" ..	4 0 "	39°5	37°0	2°5	61°5	22°0				
" ..	4 30 "	39°0	37°0	2°0	61°5	22°5				
" ..	5 0 "	38°5	37°0	1°5	61°0	22°5				
" ..	5 30 "	38°5	36°7	1°8	61°0	22°5				

## GEOLOGICAL SOCIETY.

[Continued from p. 316.]

January 24, 1872.—Joseph Prestwich, Esq., F.R.S., President,  
in the Chair.

The following communications were read:—

1. "On the Foraminifera of the Family Rotalinæ (Carpenter) found in the Cretaceous Formations, with Notes on their Tertiary and Recent Representatives." By Prof. T. Rupert Jones, F.G.S., and W. K. Parker, Esq., F.R.S.

The authors enumerated the Rotalinæ which have been found in the Cretaceous rocks of Europe, and showed by tabular synopses the range of the species and notable varieties in the different formations of the Cretaceous system. For the comparison of the Tertiary Rotalinæ with those of the Cretaceous period the following Tertiary formations were selected:—The Kessenberg beds in the Northern Alps, the Paris Tertiaries, the London Clay, the Tertiary beds of the Vienna Basin, and the English and Antwerp Crags. The authors also enumerated the recent Foraminifera of the Atlantic Ocean.

The authors stated that of *Planorbulina* several species and important varieties of the compact conical form occur throughout the Cretaceous series, and that those of the Nautiloid group are still more abundant. The plano-convex forms are represented throughout the series by *P. (Truncatulina) lobatula*; but the flat concentric growths had not yet come in. *Planorbulina* extends down to the Lias and Trias. *Pulvinulina repanda* is feebly represented in the uppermost Chalk; but forms of the "*Menardii*" group abound throughout the series. Species of the "*elegans*" group are peculiarly characteristic of the Gault; and some of the "*Schreibersii*" group are scattered throughout. These two groups extend far back in the Secondary period. The typical *Rotalia Beccarii* is not a Cretaceous form; but the nearly allied *R. umbilicata* is common. *Tinoporos* and *Patellina* occur at several stages; *Calcarina* only in the Upper Chalk.

The above-mentioned types are for the most part still living; but the "*auricula*" group of *Pulvinulina* is wanting in the Cretaceous series, as also are *Spirillina* and *Cymbalopora*, except that the latter occurs in the Maestricht Chalk. *Discorbina* and *Calcarina* make their first appearance in the uppermost Chalk. The chief distinction between the Cretaceous and the existing Rotalinæ was said to consist in the progressively increasing number of modifications. The authors concluded by disputing the propriety of regarding the Atlantic ooze as homologous with the Chalk.

2. "On the Infra-lias in Yorkshire." By the Rev. J. F. Blake, M.A., F.G.S.

The Infra-lias, *i. e.* the zones of *Ammonites planorbis* and *Am. angulatus*, have been recorded hitherto only from Redcar, to the beds at which place the author referred; but the chief object of the paper was to describe some sections at Cliff, near Market Weighton, where these and lower beds are well exposed, and have yielded a



numerous suite of fossils. He considered, however, that these beds did not belong to the typical Yorkshire area, but were the thin end of the series which stretches across England. He supposed there had been a barrier in Carboniferous times, which had separated the coal-fields of Yorkshire and Durham, prevented the continuity of the Permian beds, and curved round the secondary rocks to the north of it, to form the real Yorkshire basin, while these beds at Cliff were immediately to the south of it.

The sections described were six in number, the first pit yielding the great majority of the fossils, and the third showing best the succession of the beds. The fossils could be mostly identified with known forms, and showed a striking similarity to the Hettangian fauna. In all the clays of the Infra-lias, Foraminifera were numerous and varied.

The section in pit No. 3 showed, commencing at the top :—1. Stone bed with *Am. angulatus* (the fossiliferous bed of pit No. 1). 2. Thick clays, with bands of stone characterized by *Am. Johnstoni*. 3. One band of clay with *Am. planorbis*. 4. Thin-bedded stones and clays, some of them oyster-bands. 5. Clays without Foraminifera, and with impressions of *Anatina* (White Lias).

The *Avicula-contorta* series is not reached; nor are there any signs of the bone-bed, as the junction with the Keuper marls, which are found three miles off, is not seen.

The paper was followed by references to the fossils mentioned, including the description of those that are considered new.

### LXVIII. Intelligence and Miscellaneous Articles.

RESEARCHES ON THE REFLECTION OF HEAT AT THE SURFACE OF  
POLISHED BODIES. BY M. P. DESAINS.

WHEN, with rock-salt apparatus, we form the spectrum of lime or of incandescent platinum, we find at the least refracted extremity of those spectra lines completely absorbable by thin layers of water or even of glass. These lines are reflected at the surface of polished metals in much greater proportion than the red lines in their vicinity; and in all these characters they very nearly resemble those emitted by lamp-black heated to only 200° or 300° C. It has moreover been long admitted that the heat from obscure sources is remarkably less refrangible than luminous heat in its passage through a prism of rock-salt.

Some years since I studied the reflecting action of glass and rock-salt on rays from a source at a low temperature; and I ascertained that when the incident flow was completely polarized in the plane of incidence, Fresnel's formula  $I^2 = \frac{\sin^2(i-r)}{\sin^2(i+r)}$  represents exactly the course of the phenomenon:  $i$  is the angle of incidence, always connected with the angle  $r$  by the relation  $\sin i = n \sin r$ . In the

case of rock-salt,  $n$  is the index corresponding to the rays employed. It appeared to me to require to be taken as equal to 1.49. In the case of glass, the value of  $n$  which satisfies experiment is  $n=1.7$ ; but here measurements of the index are not possible, because glass even of the thickness of  $\frac{1}{2}$  millim. obstructs the passage of the rays employed. If, however, we suppose that, in this case again,  $n$  retains its ordinary physical signification, we must conclude that in glass the extreme obscure rays are incomparably more refracted than the red rays, which are nevertheless much more refrangible than the former in a rock-salt prism.

These facts were recorded in a memoir presented to the Academy in 1868. I have thought it necessary to mention them now in order to compare them with the experiments made eighteen months since on abnormal dispersion, and with those which M. Leroux had previously published on the same subject.

The study of reflection at the surface of metals leads to analogous consequences. On studying, with De La Provostaye, the reflection of polarized heat at metallic surfaces, we have recognized that Cauchy's formulæ very well represent the results obtained for the two principal positions of the plane of polarization.

But when limited to the case in which the plane of polarization is parallel with that of incidence, we may be confident that the formula  $I^2 = \frac{\sin^2(i-r)}{\sin^2(i+r)}$  very well represents the data of experiment, the angle  $r$  being always connected with the angle of incidence by the formula  $\sin i = n \sin r$ ,  $n$  being a constant. But the values of  $n$  are in general very considerable; for the rays in the vicinity of the extreme red they appeared to me to be the following: platinum,  $=8$ ; speculum-metal,  $=8.7$ ; silver,  $=20$ ; finally  $n=26$  in the case of rays emitted by lamp-black at  $300^\circ$  and reflected by speculum-metal. This enormous increase agrees well with what I had already found in studying the reflection from glass.

When the rays are polarized perpendicularly to the plane of incidence, Fresnel's formula  $I^2 = \frac{\tan^2(i-r)}{\tan^2(i+r)}$  no longer represents either the metallic reflection or that of rays of low temperature from glass. But the considerations from which that formula is derived require very little modification in order to yield one which will represent the phenomena.

Indeed, let us suppose that the *vis viva* of the refracted pencil, instead of being equal to the difference between that existing in the direct and that existing in the refracted pencil, differs a little from that excess, and we shall then have between the coefficients of vibration  $i$ ,  $v$ , and  $u$  of the three pencils a relation of the form

$$(1-v^2) \cos i \sin r = u^2 \cos r \sin i (1-\delta),$$

$\delta$  representing the coefficient of the corrective term. Now I have ascertained that by the introduction of this term we arrive at a final formula which well represents all the experiments I am acquainted with, provided that we assign to  $\delta$  a value of the form  $k \tan^2(i-r)$ ;

$k$  is a constant depending on the nature of the rays and on that of the mirror. The constant  $n$ , always defined by the relation  $\sin i = n \sin r$ , retains the value deduced from the experiments in which the plane of polarization is parallel with that of incidence.

The final formula is the following :—

$$\frac{\text{tang}(i-r)}{\text{tang}(i+r)} = -I \left[ 1 + \frac{\cos i \sin i k \text{tang}^2(i-r)}{\cos i \sin i + \cos r \sin r} \right] + \frac{\cos i \sin i k \text{tang}^2(i-r)}{\cos i \sin i + \cos r \sin r}.$$

I have verified this formula between the incidences of  $20^\circ$  and  $75^\circ$ .  
—*Comptes Rendus de l'Acad. des Sciences*, April 22, 1872.

PRIZE QUESTION PROPOSED BY THE DANISH ROYAL SOCIETY OF SCIENCES FOR THE YEAR 1872.

The detailed researches of which the spectra of the planets have been the subject since the introduction of the spectroscope in astronomical investigation are yet far from having led to satisfactorily accordant results, even in respect of the principal points. At this moment we know positively only one thing—namely, that the spectra are not at all identical with that of the solar light; while great uncertainty, even actual contradiction, prevails when the positions of new lines and zones of absorption have to be determined; these in the case of Uranus, for example, seem to change completely the character and nature of the spectrum. It is true that these researches, in order to have all the precision desirable, demand observations which are some of the most difficult and delicate in astronomy; but the above-mentioned discordances, and notably those which belong to the researches previous to 1868, must certainly be attributed in part to the entire absence of a normal general spectrum-scale, such as was given some years since by M. Ångström in his celebrated work.

In the conviction that the analyzers and instruments of precision now available permit the spectroscopic examination of the planets Venus, Mars, Jupiter, Saturn, and Uranus in such a manner that doubts shall no longer be possible relative to the position and special nature of the principal lines, groups, and zones in each of these spectra, the Danish Royal Society requests a description of the spectra of these planets, accompanied by a critical comparison of the results obtained anteriorly by Dr. William Huggins, Father Secchi, Dr. Vogel, and, particularly as regards Jupiter, by Mr. Le Sueur at Melbourne, and offers as the prize its gold medal together with a sum of money representing the value of that medal.

The memoirs in answer to this must be sent in before the end of October 1873, addressed to Councillor Japetus Steenstrup, Secretary of the Society. They may be written in Latin, French, English, German, Swedish, or Danish. The memoirs must not bear the names of the authors, but must be furnished with mottoes; and



each memoir must be accompanied by a sealed packet bearing on the outside the same motto as the memoir, and enclosing the name, profession, and address of the author. The value of the gold medal is stated at 450 francs.

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ANOMALOUS PRODUCTION OF OZONE. BY HENRY H. CROFT,  
PROFESSOR OF CHEMISTRY, UNIVERSITY COLLEGE, TORONTO.

About six years ago, when evaporating some syrupy iodic acid, prepared according to Millon's process, over sulphuric acid, I noticed that when the acid began to crystallize the air in the jar (covering the drying-dish) had a strong smell of ozone or active oxygen. A couple of years afterward, on again making iodic acid, this observation recurred to my mind, and I carefully tested the air in the jar during the evaporation; no trace of ozone could be detected until the acid began to crystallize, when the smell of ozone became immediately perceptible, and all the usual tests for that body succeeded perfectly.

During the last month I have had occasion to convert two ounces of iodine into iodic acid; and exactly the same result has been observed.

The acid usually solidifies to opaque verrucose masses; but on this occasion the crystals formed were clear and brilliant. The solution had in this, as in all the former cases, been boiled down to thin syrup, so that no trace of chlorine or nitric acid could possibly have remained to act on the ozone paper. The air in the jar was tested from day to day both by the smell and the action of iodized starch-paper. Even when a few crystals began to form, no change was noticed; but when the crystallization set in fully, the evolution of ozone was most remarkable, the strong smell being quite characteristic, entirely different from that of chlorine or nitric acid.

I am quite unable to account for this ozonification of the air (or oxygen) over crystallizing iodic acid. My friend, Mr. Sterry Hunt, has suggested that it may arise from a partial deoxidation similar to that which produces ozone when hypermanganates are decomposed, as observed by him and other chemists. As the crystallizing acid remains perfectly white, either opaque or transparent, and as the lower oxides of iodine are of a yellow, or even brown colour, according to Millon, I cannot accept this explanation; and even if it were true, the phenomenon would be equally unintelligible, a reduction taking place during crystallization. I can offer no explanation of the *simple fact* that air over crystallizing pure iodic acid becomes ozonized; but I think that the observation seems to offer a wide field for further experiments, which I have unfortunately not the time to carry out.—Silliman's *American Journal* for June 1872.

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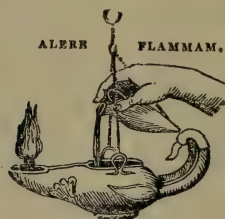
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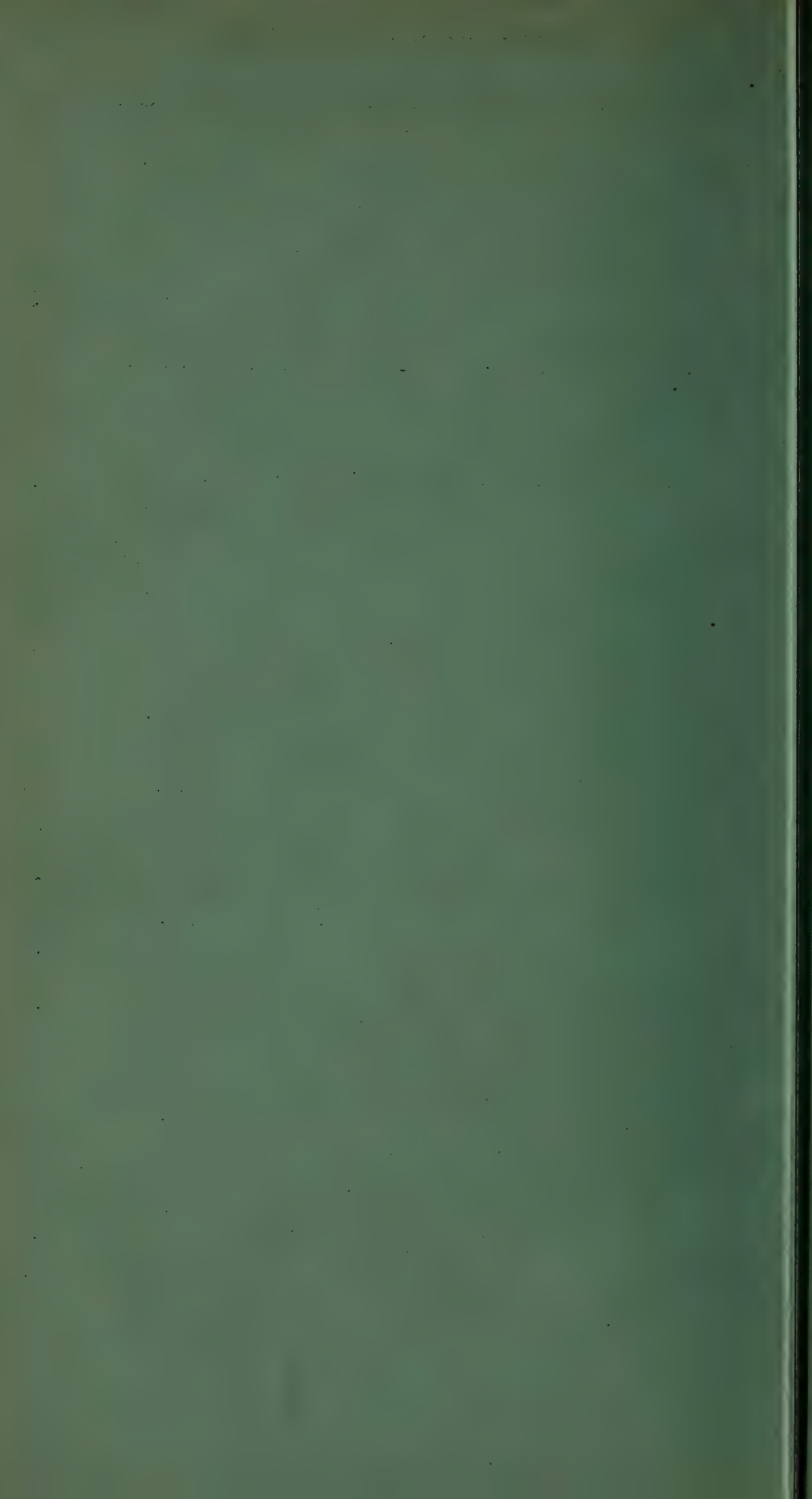
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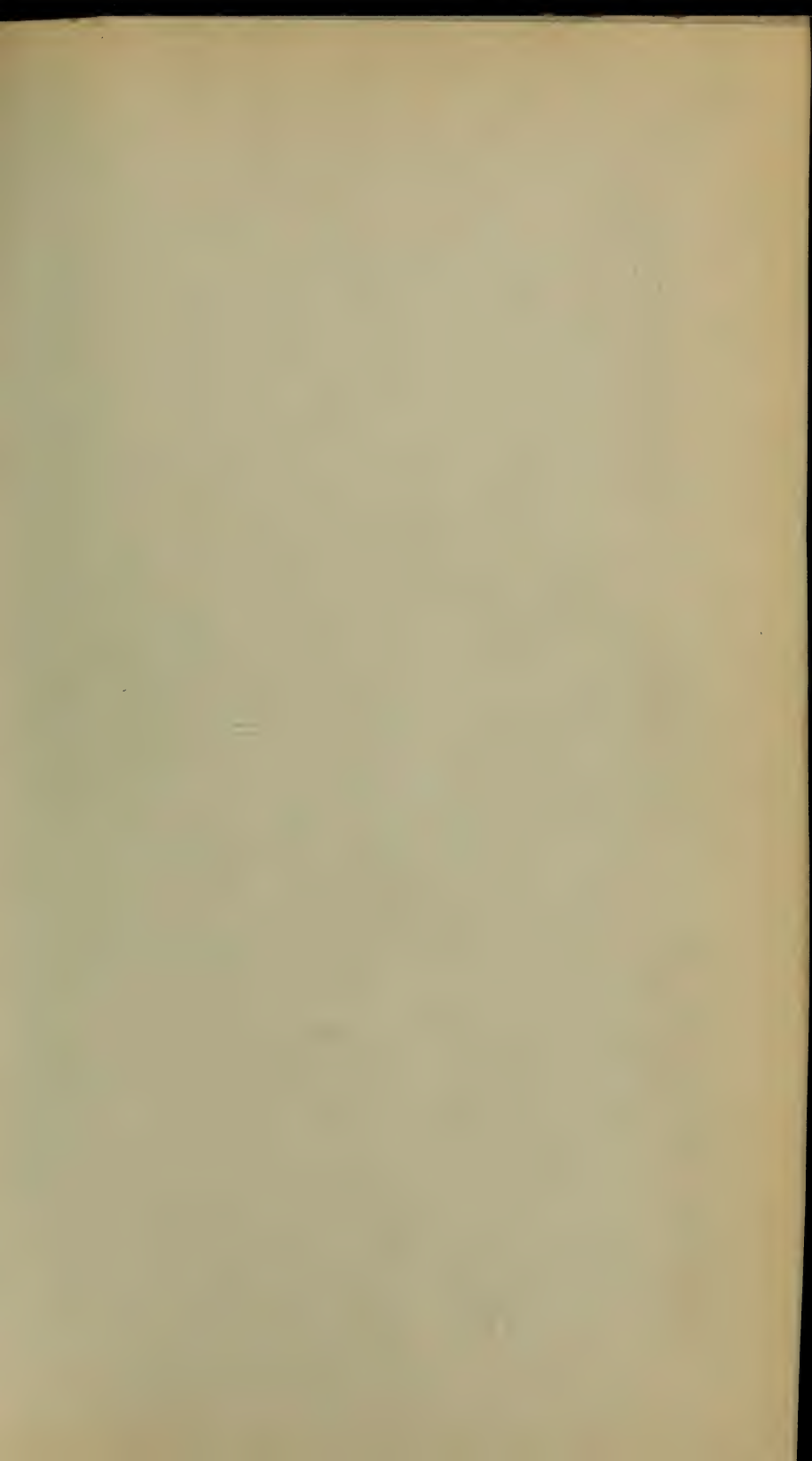
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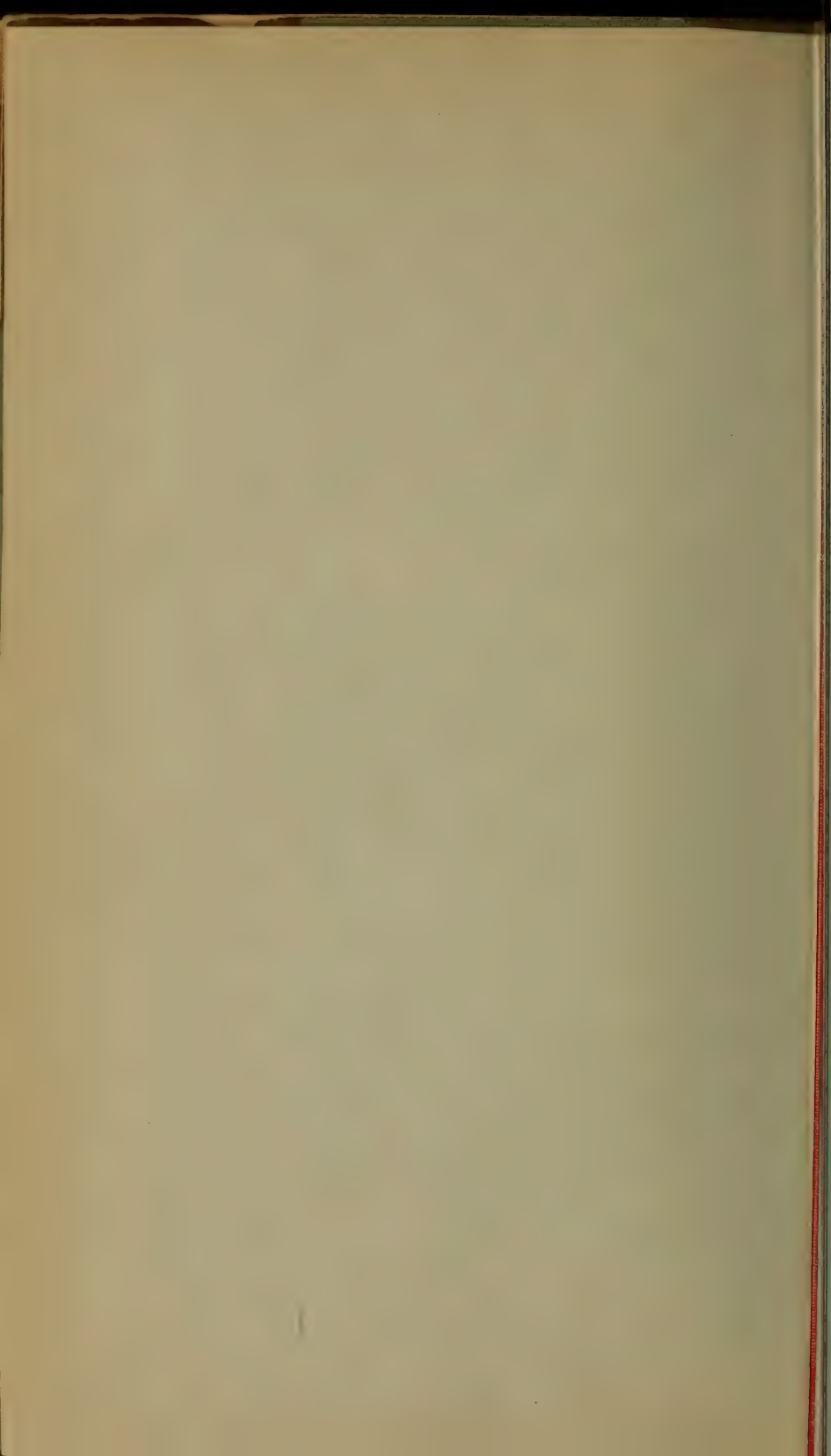
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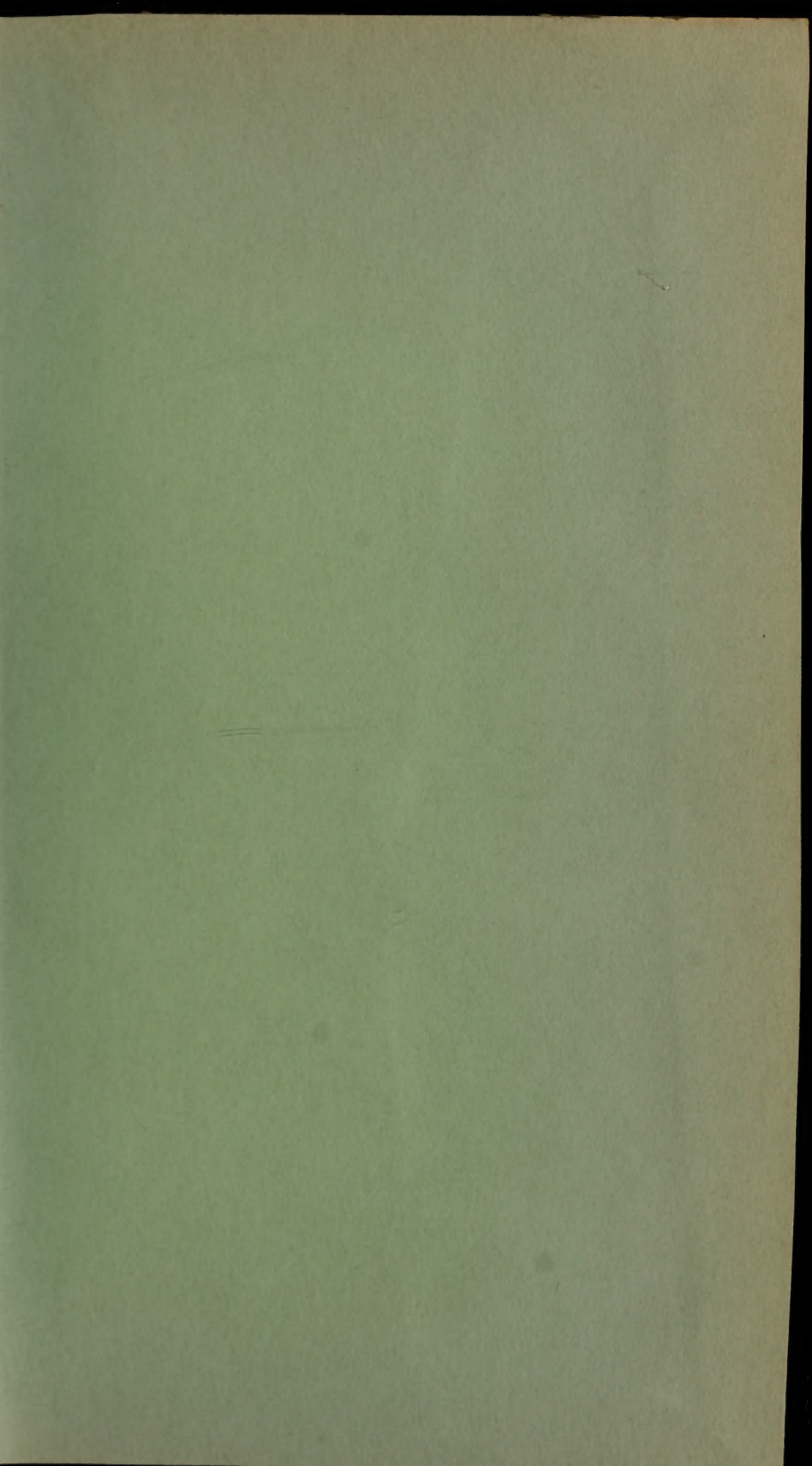
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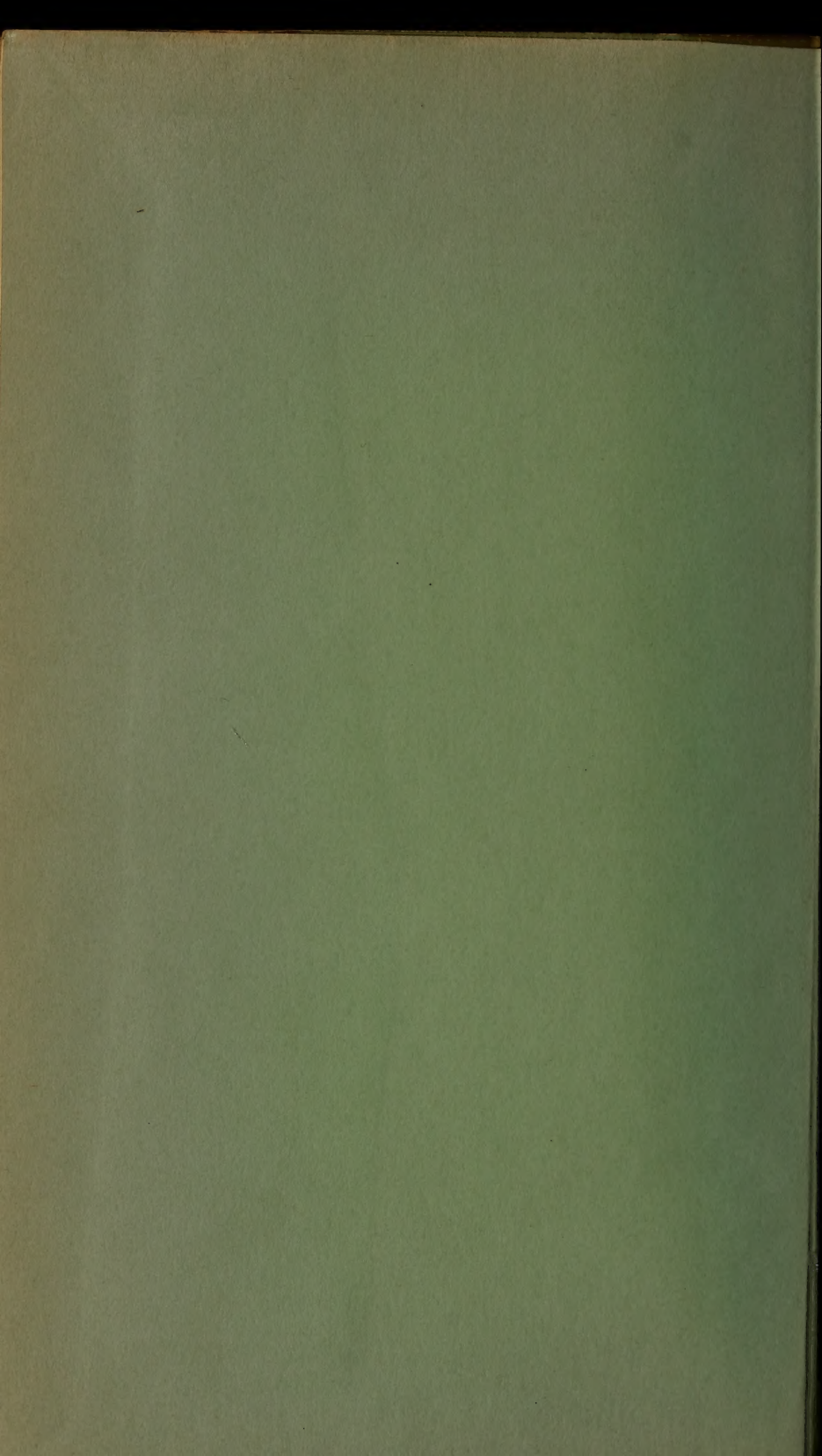
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